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ON THE ANGULAR SPEED OF ROTATION OF A LONG-ENDURING PROMINENCE

By J. EVERSHED

The prominence described by Dr. Slocum in the *Astrophysical Journal* for September 1910 (**32**, 125), and figured in Plate XII, was photographed with the Kodaikánal spectroheliograph at each successive appearance on the sun's limb. Our photographs are very similar to those taken at the Yerkes Observatory, but, contrary to Dr. Slocum's experience, our K_2 flocculi plates show the prominence also as an absorption marking on the disk of the sun at three successive meridian transits. On the three days following March 22 the prominence is such a conspicuous and remarkable object on the disk that it is difficult to understand Dr. Slocum's failure to photograph it.

Taking advantage of the exceptional opportunity afforded by the disk photographs for determining the speed of angular rotation of the prominence, I have made a series of measures of a well-defined portion of the absorption marking, and the results of these measures, together with a spectrographic determination of rotation speed, are, I think, of sufficient interest to give in some detail.

As the spring months at Kodaikánal are the most favorable for solar work, our series of photographs is very complete, and they show that the prominence endured in a more or less compact form for at least 82 days, and the region of longitude in which it

was situated became very active again a month later on May 24 and 25.

I give in the following table the dates when the prominence had attained its greatest apparent development on either limb, and the approximate limits of latitude covered.

Date	Limb	Latitude	Height
1910 Feb. 5.....	West	+ 2° to −14°	55″
Feb. 19.....	East	0 to −22	35
March 5.....	West	− 6 to −20	95
March 18.....	East	+15 to −20	100
April 1.....	West	− 9 to −23	40
April 14.....	East	+ 5 to −25	50
April 28.....	West	− 7 to −23	80

Excepting during the early stages of development in February the prominence was visible for three successive days at each limb, and the dates given are the second day of visibility in each case, when the maximum apparent height was reached. The prominence was very variable in extent, and it is not easy to assign exact limits in latitude, on account of small outlying prominences apparently connected with the main mass by faint streamers. When on the eastern limb, it was always more extended northward than when on the west limb.

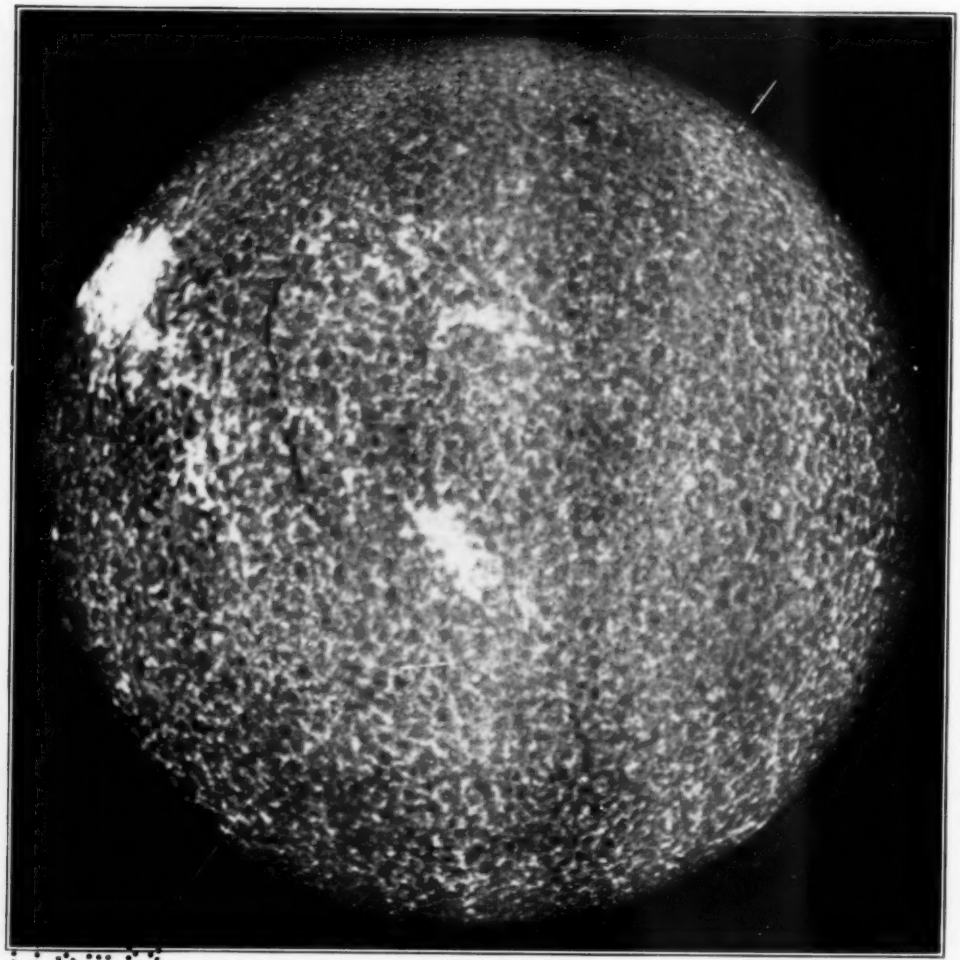
The general aspect of this "quiet" type of prominence and the rapid changes constantly in progress in the smaller details suggest a continuous renewal of the glowing gas from numerous orifices situated within the photosphere. If this be so, it is evident that in deducing values of angular rotation from successive apparitions on opposite limbs we really obtain the rotation speed of these orifices, not that of the chromosphere. But the chromospheric layer, according to the researches of Adams, has an angular rotation speed at the equator of about $6\frac{1}{2}$ per cent greater than that of the photosphere as deduced from spots and faculae: consequently the prominence gases projected into the chromosphere will constantly tend to drift westward over the photosphere, like smoke driven by the wind.

The velocities of angular rotation that I have obtained by different methods for the prominence under discussion appear to show distinctly that this westward drifting really takes place, while

PLATE I

North Pole

W



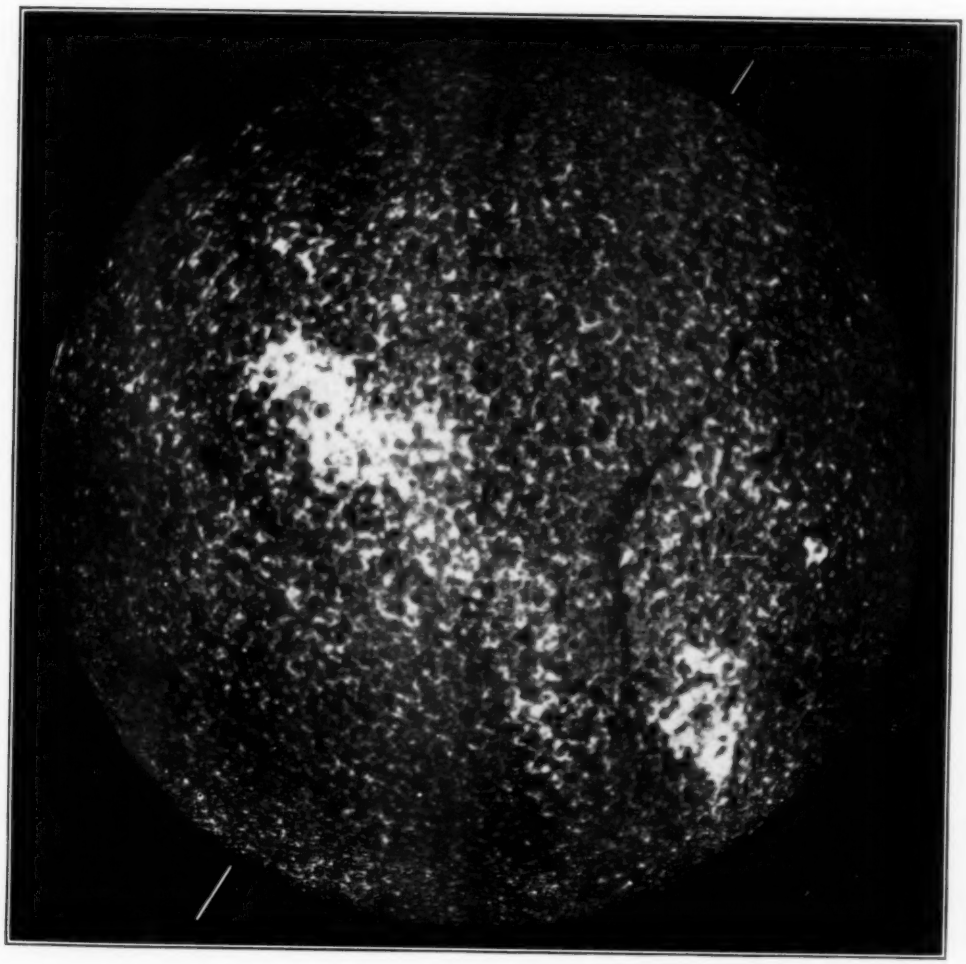
February 28, 1910

SPECTROHELIOGRAMS SHOWING D

Taken with calcium line K. Kodail

PLATE II

North Pole



E

March 23, 1910

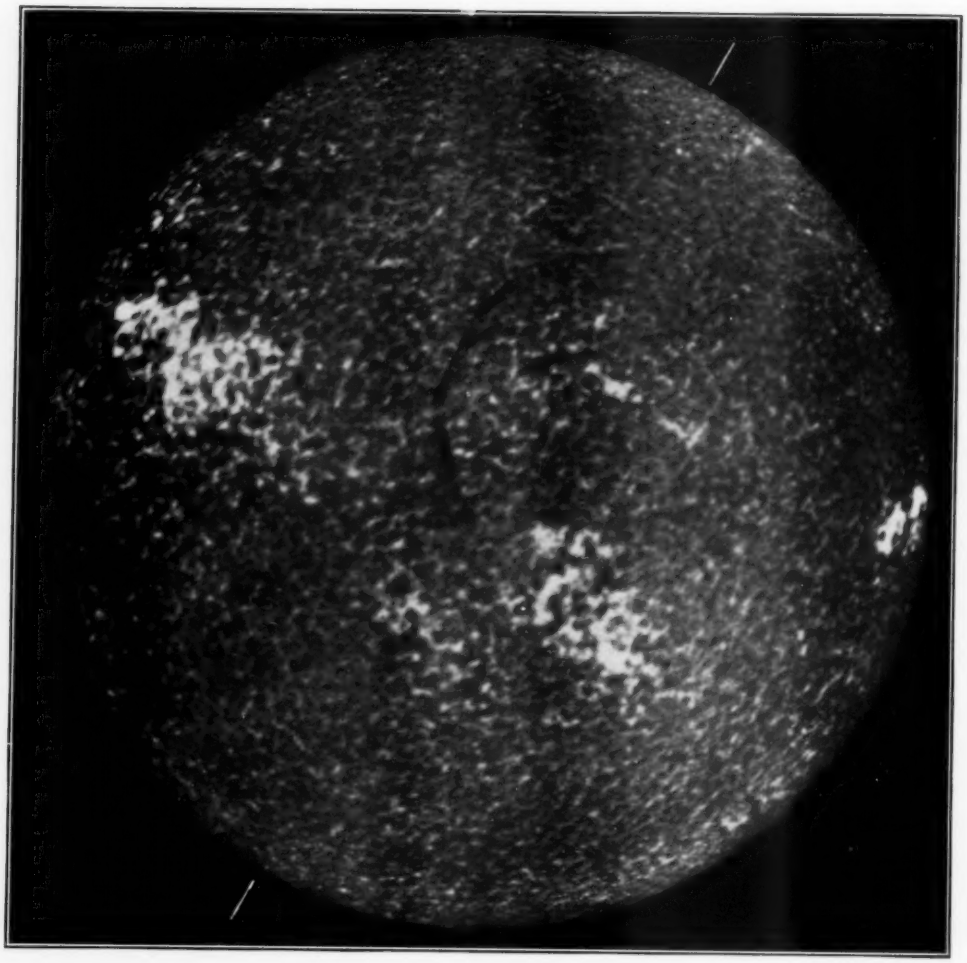
SHOWING DARK FLOCCULUS

K. Kodaikánal Observatory

PLATE III

North Pole

W



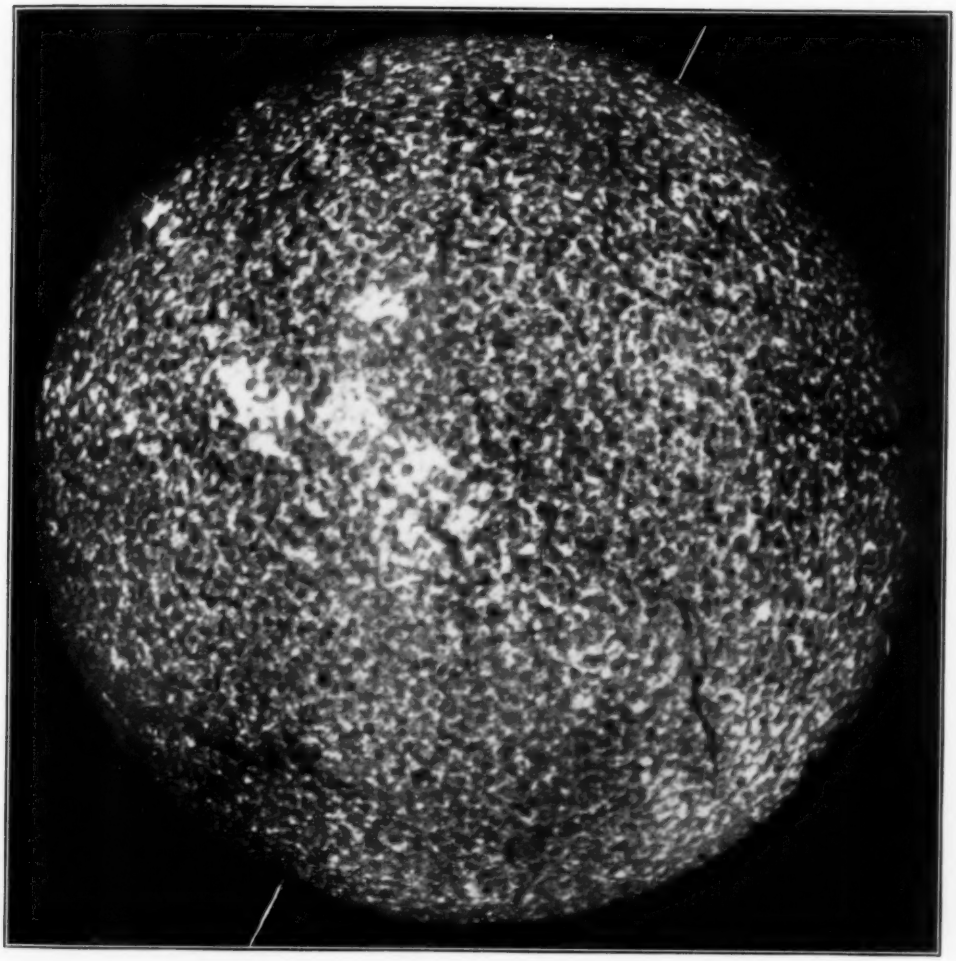
March 25, 1910

SPECTROHELIOGRAMS SHOWING

Taken with calcium line K_2. Kod

PLATE IV

North Pole



E

April 18, 1910

S SHOWING DARK FLOCCULUS

ne K_3. Kodaikánal Observatory

the locality of origin of the prominence rotates with the normal speed of the photosphere.

If we take the interval of 82 days between February 5 and April 28 to represent three synodical revolutions of the prominence, we get a rotation speed of 13°.2 per diem, or a sidereal speed, $\xi = 14°.2$ per diem. This corresponds closely with the angular speed of spots in latitude 10° to 15°. The mean latitude of the prominence at its western apparitions is $-13°$.

The estimate of the time when a prominence is at the limb or 90° from the central meridian is very uncertain, however, and the whole interval may well be subject to an error of ± 1 day, so that the above value may be over 1 per cent in error.

A much more accurate estimate is obtained by measuring the absorption marking due to the prominence projected on the disk, and deducing the times of passing the central meridian at the successive transits. The marking was photographed on February 25, 26, 27, and 28; on March 22, 23, 24, and 25; and again less distinctly every day from April 16 to 26 inclusive. In the two earlier apparitions it is shown as a bow-shaped dark streak, crossing the equator (see Plates I and II), the center of the bow lying on the equator. During February the bow was narrower than in March, and more sharply bent at the center, forming an obtuse angle; this definite point, which was in latitude +0°.4, was measured on the successive days, giving a good value of the daily motion, as well as the time of meridian passage. The three plates of March were similarly measured, but in this case a point had to be chosen 8° north of the equator, where the marking contracted to a narrow line, nearly perpendicular to the equator. In April only the southern arm of the bow can be seen, highly inclined to the equator, and accurate measures of the daily motion are not possible; but from measures of the western end of the marking in the plates of April 20, 22, and 23 I have estimated the time of meridian passage of a point in latitude $-3°$.

The times of meridian transit deduced from these measures are

1910 February	25, 9^h	12^m	Greenwich Civil Time
March	24, 7	20	Greenwich Civil Time
April	20, 3		Greenwich Civil Time

The first interval of 26.92 days, representing a complete synodical revolution, is equal to a mean daily sidereal motion, $\xi = 14^\circ.37$; and the second interval of 26.84 days is equal to a mean motion of $14^\circ.40$. The first interval is not likely to be in error by an amount exceeding one part in 400, but the second is less trustworthy. These values may be taken to represent the equatorial velocity, notwithstanding the fact that the measures for March refer to a point in latitude $+8^\circ$.

It is assumed, of course, that the marking photographed in February is identical with those obtained in March and April; this is probably true only in the same sense that the prominence photographed on the west limb on February 5 is identical with those photographed on later dates; that is to say, the angular velocity obtained is the velocity of the origin of the marking, not that of the absorbing gas itself. It is in quite remarkable agreement with the equatorial speed of the photosphere as determined from spots.

The measures of angular motion from day to day give quite a different result, as the following determinations of longitude measured from the central meridian clearly indicate.

DAILY MOTION IN LONGITUDE OF ABSORPTION MARKING SITUATED IN LATITUDE $+0^\circ.4$

Date	G.C.T.	Longitude	Motion in 24 Hours	ξ
1910 Feb. 25	$2^h 40^m$	$3^\circ.8$ East	$14^\circ.00$	$15^\circ.00$
Feb. 26	2 42	10.2 West	14.12	15.12
Feb. 27	2 45	24.4 West	14.14	15.14
Feb. 28	3 01	38.7 West		

Mean daily sidereal motion $= 15^\circ.09$

DAILY MOTION IN LONGITUDE OF ABSORPTION MARKING SITUATED IN LATITUDE $+8^\circ$

Date	G.C.T.	Longitude	Motion in 24 Hours	ξ
1910 March 23	$2^h 31^m$	$18^\circ.3$ East	$15^\circ.3$	$16^\circ.3$
March 24	2 42	2.6 East	14.8	15.8
March 25	2 21	11.7 West		

Mean daily sidereal motion $= 16^\circ.05$

As a check on these results I measured at the same time a small bright flocculus, which could be identified in successive plates during four days; the results are as follows:

DAILY MOTION IN LONGITUDE OF A CALCIUM FLOCCULUS SITUATED IN LATITUDE −8°

Date	G.C.T.	Longitude	Motion in 24 Hours	ξ
1910 March 23.....	$2^h 31^m$	41°.2 East		
			12°.9	13°.9
March 24.....	2 42	28.2 East		
			14.1	15.1
March 25.....	2 21	14.3 East		
			13.3	14.3
March 26.....	2 24	1.0 East		

Mean daily sidereal motion = 14°.43

The above result, although somewhat rough, is in substantial agreement with Hale's determination of mean motion of the flocculi,[1] and shows that the absorption marking was drifting westward with respect to the flocculus.

It appears, then, that the dark mass of calcium vapor (and hydrogen) near the equator had an angular rotation speed 5 per cent greater than the general surface of the photosphere during the February apparition, and as much as 11 per cent greater during the March apparition. Also that the two apparitions really represent two distinct masses of absorbing gas, emanating from a common origin approximately in solar longitude 75°. Although the February marking could be traced nearly to the western limb on March 3, it must have become dissipated subsequently, as it was not shown near the east limb on some excellent plates taken on March 19 and 20. On March 21 it reappears as a vague and ill-defined dark mass, extending across the equator, and some distance within the east limb. On the following day the bow-shape became evident.

The intermittent character of the marking is also shown in its later phases. On March 25 it had attained its greatest apparent development, extending for a distance of at least 36° of solar latitude, or 250,000 miles (400,000 km). The northern arm can indeed be faintly traced for a much greater distance in a vast circular sweep toward the eastern limb, which it nearly reaches. Notwithstand-

[1] *Astrophysical Journal*, **27**, 227, 1908.

ing the prodigious length of this mass of relatively cool gas, the whole object utterly vanished during the next 24 hours. The plate of March 26 is of the best quality, and shows two small prominences as dark markings on the disk, yet not a trace can be seen of the large marking, nor is it shown on the plates of the 27th or 28th. A portion of the southern branch is seen again on the 29th, but in a more easterly position on the disk; this apparently reappears on the east limb on April 16, and continues visible until near the west limb on the 26th of the month.

From the whole inquiry it seems definitely established that there was a region on the sun, narrowly defined in longitude, and partaking of the normal rotation speed of the photosphere, which was somewhat intermittently giving rise to prominences, and that these prominences drifted westward with about the angular speed of the hydrogen of the chromosphere. The remarkable bow-shape of the absorption marking, with the center over the equator, is very suggestive of a wave surging westward over the photosphere, with a greater speed near the equator than in higher latitudes. Yet the velocity obtained in latitude $+8°$ in March is distinctly greater than that observed on the equator a month earlier. The marking is unfortunately too indefinite in higher latitudes for measures of longitude to be made. It is perhaps more probable that the bow-shape is really due to the original disposition of the prominence-forming orifices.

The mean level of the absorbing calcium vapor is difficult to determine. If the whole prominence is effective in producing absorption it may be roughly estimated at some 30″ above the photosphere, but there is reason for supposing that only portions of the prominence are cooled sufficiently to be effective, and these portions may be in the lower and denser region not extending much above the chromosphere. When these dark markings are seen extending to the sun's limb they are found almost invariably to end in a prominence, but the latter in many cases are somewhat insignificant in height. On the other hand, many large prominences, perhaps the majority of them, seem to produce no absorption on the disk.

The rotational movement of the higher regions of a "quiet"

prominence can be ascertained by spectrographic measures, and this method of investigation has for some time past been on our program of research. Only a few plates have as yet been obtained, but these fortunately include the prominence of March when on the east limb. With a radial slit placed across the limb at the sun's equator the *Ha* line was photographed in this prominence on March 17 and 18. It appears as a sharply defined bright line, about 0.5 mm wide outside the limb, and as an absorption line 1 mm wide within the limb, the scale of the plates being 1 mm = 1.2 Å. I have measured the displacement of the bright line with respect to the dark line, and the result distinctly confirms the westward movement of the prominence. In measuring the plates a single straight thread was used, placed parallel to the spectrum lines, and the error in parallelism which might seriously affect the result was carefully determined and allowed for. The mean results obtained, with the red end of the spectrum to the right and left respectively, are as follows:

Date	Height above Photosphere	$\Delta\lambda$	Km per Sec.
1910 March 17...	17″	−0.015 Å	+0.68
March 18...	30	−0.015 Å	+0.68

The absorption line may be taken to represent the normal chromosphere line, with a velocity of approach of about 2 km per second. The excess velocity of the prominence at a considerable height above the chromosphere amounts therefore to as much as 34 per cent. The consistence of the measures with the red end of the spectrum to right and left, and the undistorted character of the emission line lead me to believe that this relatively high velocity is real and permanent. It would be unsafe, however, to infer from a single instance that the law of increase in angular speed with height discovered by Adams is continued outside the limits of the chromosphere, but this may be taken as a suggestion only.

SOLAR OBSERVATORY
KODAIKÁNAL, SOUTH INDIA
November 29, 1910

PHOTOGRAPHIC DETERMINATIONS OF STELLAR PARALLAX MADE WITH THE YERKES REFRACTOR. II

By FRANK SCHLESINGER

Having described the process at the telescope by which the plates were obtained, it is now in order to consider the methods to be employed

AT THE MEASURING ENGINE.

The choice between polar and rectangular co-ordinates for parallax work is easily made: the former are more difficult to measure, they entail more cumbersome computations, and they restrict the choice of comparison stars. The last of these disadvantages is especially marked when distances only are considered, but it is of consequence even when both position-angles and distances are measured. In rectangular co-ordinates a comparison star in one position is practically as good as in any other, if only the mean position of all the comparison stars (sometimes referred to as their "center of gravity") is not too far from the parallax star.

Having then decided to use rectangular co-ordinates, we must next say whether we shall measure in one or in two directions. If a star is near the ecliptic its parallactic ellipse is narrow, and little additional information is gained by measuring the displacement in latitude. Furthermore, by observing early in the evening and late into the morning, plates may be obtained at the times when the star is at either extremity of the major axis of the parallactic ellipse. But it is impossible, except in very rare cases, to obtain observations at both ends of the minor axis, since one of them corresponds to the time when the longitude of the sun is equal to that of the star, and therefore when the latter culminates not far from noon. This fact alone reduces the weight of the parallax as derived from latitude displacements nearly in the ratio of 1 to 4, as compared with that from longitude displacements. For eighteen of the stars here discussed we have therefore oriented the axis parallel to the ecliptic and have measured longitude co-ordinates only.

This is in general the direction that yields the maximum weight for the deduced parallax.

It seemed, however, particularly desirable that, for at least some of the stars, the parallax should be derived independently from two co-ordinates; this has accordingly been done for the seven stars in the present list that are best suited to the purpose. But instead of measuring in longitude and latitude, it is a little preferable to measure in right ascension and declination. The results show that measures in declination have somewhat smaller probable errors than those in right ascension, and therefore the two should be kept separate. This difference in accuracy is doubtless due to the guiding error, the following in declination being independent of the behavior of the clock.

Whatever direction may be chosen for the axes, the combined weight for the two resulting determinations of a parallax is always the same. To prove this, let l and b represent the "parallax factors" in longitude and latitude at the time t; that is, the two displacements due to the parallax (π) will be $l \cdot \pi$ and $b \cdot \pi$. Each plate will, therefore, offer equations of this form:

$$\begin{aligned} c+t_1\cdot\mu+l_1\cdot\pi&=M_1 & k+t_1\cdot\nu+b_1\cdot\pi&=N_1\\ c+t_2\cdot\mu+l_2\cdot\pi&=M_2 & k+t_2\cdot\nu+b_2\cdot\pi&=N_2\\ \text{etc.,}& & \text{etc.,}& \end{aligned}$$

where c and k are constants and μ and ν are the proper-motion components. The zero of time may be so chosen as to make the sum of the times for all the plates equal to zero. Accordingly the normal equations for the longtitude measures are

$$\begin{aligned} n\cdot c+O\ \mu+[l]\quad \pi&=[M]\\ +[t^2]\mu+[t\cdot l]\pi&=[t\cdot M]\\ +[l^2]\quad \pi&=[l\cdot M] \end{aligned}$$

where n is the number of plates or equations. The weight of π derived from this solution is

$$[l^2]-\frac{[l]^2}{n}-\frac{[l\cdot t]^2}{[t^2]};$$

and similarly the weight from the measures in latitude is

$$[b^2]-\frac{[b]^2}{n}-\frac{[b\cdot t]^2}{[t^2]}.$$

Now let us suppose that the plates are measured anew, with axes rotated through any angle ϕ; then the two displacements due to parallax will be

$$l\cdot\cos\phi-b\cdot\sin\phi \quad \text{and} \quad l\cdot\sin\phi+b\cdot\cos\phi.$$

The observation equations in one direction are

$$c^{1}+t_1\cdot\mu_1{}^{1}+(l_1\cdot\cos\phi-b_1\cdot\sin\phi)\pi=M_1',$$
$$c^{1}+t_2\cdot\mu_2{}^{1}+(l_2\cdot\cos\phi-b_2\cdot\sin\phi)\pi=M_2',$$
$$\text{etc.},$$

with similar expressions for the other co-ordinate. Deriving as before the two weights for the parallax, and adding them together, we obtain, after some simple reductions,

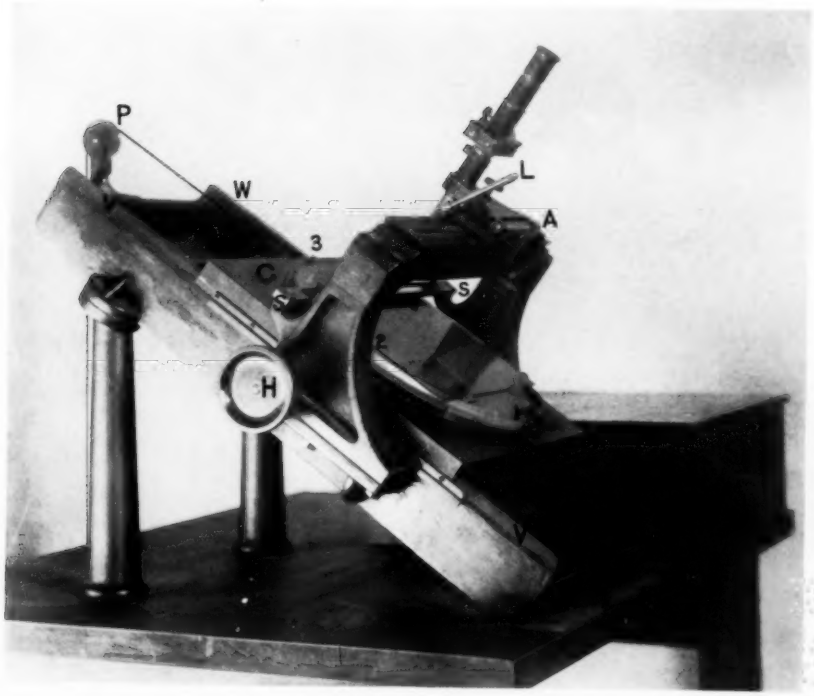
$$[l^2]+[b^2]-\frac{[l]^2+[b]^2}{n}-\frac{[l\cdot t]^2+[b\cdot t]^2}{[t^2]},$$

which is equal to the sum of the weights derived from the measurements in longitude and latitude.[1] There is thus no loss of total weight in measuring the displacements in right ascension and declination. This procedure yields two determinations that are in general more comparable in reliability than the two that would follow from measures in longitude and latitude.

Specifications for a measuring engine to meet the requirements just outlined were drawn up by the writer, and intrusted to Wm. Gaertner & Co., of Chicago, by whom they were carried out in a satisfactory manner. The photograph to be measured is clamped to the carriage C (Plate V) in such a way as to bring the film always into the same plane. To put a star under the micrometer microscope the carriage may be moved to and from the observer, and the microscope itself may be racked to the right or left. The former motion is executed by means of the handle H, which has its duplicate on the right side of the engine so that either hand may be employed for the purpose. It is necessary that the motion should be accurately in a straight line, and the V-shaped guiding way (VV), 67 cm long, was scraped with this in view. On the lower side of the carriage and to the left of the observer are two V-shaped

[1] As this demonstration makes no use of the relation between l or b and the time, it is obviously applicable to other than parallax measurements.

PLATE V



MEASURING MACHINE FOR STELLAR PARALLAX PLATES

lugs, 30 cm apart, that just fit over the guiding way. On the right side of the carriage is a third lug, distant 31 cm from the line joining the two on the left side; the surface of this lug is slightly rounded and it rides upon the plane way *WW*.

The carriage, which is inclined 35° to the horizontal, is accurately counterpoised as shown in the photograph. The point at which the counterweight cord is attached was fixed by experiment so as to make the pull go through the center of gravity of the carriage; and the sheave *P* is so placed that the cord is nearly parallel to *VV*. There is thus little tendency for the carriage to twist as it is moved toward and from the observer.

The microscope objective is half as distant from the plate as from the cross-wires, and the image at its focus is therefore double the size of that upon the plate. The micrometer screw has a pitch of half a millimeter and consequently four turns correspond to one millimeter upon the plate. The micrometer head is graduated into one hundred parts, so that estimated tenths of these spaces are equal to quarter-microns, or 0".0027 on plates taken with the 40-inch telescope. The scale with which the plates are compared is shown at *SS*. It is divided into millimeters but numbered with every fourth number, thus: 0, 4, 8, 12, etc. This was done in order that we may add the micrometer reading directly to the reading on the scale and obtain the measurements in quarter-millimeters, without the awkward multiplication by 4 that would otherwise be necessary.

In order to compare the plate with the scale, the device employed by Repsold[1] has been adopted. This consists in tilting the microscope through a small angle around a horizontal axis parallel to the scale. The observer sets upon an image and records the micrometer reading; he then depresses the lever *L*, which rotates an eccentric not in view in the photograph, and thus tilts the micrometer around the axis *A*, which is approximately parallel to the scale. The latter is so placed that it now appears, in good focus, in the field of the microscope. The micrometer wire will in general appear between two divisions and the observer makes a bisection and reading upon the one with the lower number, even though it

[1] See Scheiner's *Photographie der Gestirne*, p. 149.

is the more distant of the two; and so for the other images that it is desired to measure.

The micrometer eyepiece has a focal length of about 8 cm; as the objective doubles the size of the stellar image, the net magnifying power is therefore about seven. This is sufficient for plates taken with the 40-inch telescope, even the smallest images (0.07 mm) being large as compared with those obtained with smaller telescopes. In order to eliminate the considerable error depending upon the diameter of the images to be measured, and arising from the tendency to set the wire always too far to the right or always too far to the left, the eyepiece is provided with a reversing prism. Two stops are so placed that the prism may be rotated just 90° and back into the original position. The field is thereby apparently reversed 180°. All bisections, whether upon the stellar images or upon the scale, are made in both positions of the prism, and only the means are used.

The complete operation of measuring an image is as follows: Bring the image into the center of the micrometer field, bisect it, and record the micrometer reading, 0.054. Tilt the microscope, bisect the next lower line, 268, and record the reading, 3.241. Reverse the prism, read again on scale, 3.247. Lower the microscope to view the plate and read again on the image, 0.062. The measured co-ordinate is therefore 271.186 quarter-millimeters. This unit has been retained until the very last step in the computations, when the parallax, its probable error, etc., are transformed into arc by applying the factor 2.66, the number of seconds corresponding to a quarter-millimeter on plates taken with this telescope.

Cases may arise in parallax work where it would be necessary to rotate the plate exactly 90° and to measure accurately the corresponding co-ordinates of the comparison stars, even though the parallax is to be deduced from the displacements in only one of these directions. Suppose, for example, that only two comparison stars are available, or that all the comparison stars are nearly in the same straight line; then it is impossible or impracticable to deduce the plate constants (orientation and scale-value) from measures of only one co-ordinate; but they may be computed, after the application of refraction corrections, if both co-ordinates have

been measured. Means for rotating the plate 90° were provided in the construction of the measuring engine, and, although none of the stars in the present list requires measurements of this kind, it will be well to complete the description of the engine in this respect.

The usual method for effecting this rotation is by the use of an accurately graduated circle read by two micrometer microscopes 180° apart. The same circle permits also of the measurement of position-angles, but these were not contemplated with this engine. As the circle complicates the construction and adds considerably to the expense, the following plan was adopted instead:[1] four posts, shown at 1, 2, 3, and 4 in the photograph, are placed at intervals of nearly 90° and with their upper surfaces in the plane occupied by the sensitive film, so that they are in the focus of the micrometer microscope. Four delicate dots, small and round, are punched into these surfaces, and form the vertices of a quadrilateral that is nearly square. The first of these dots is adjustable, so that the line joining it and (3) can be made, once for all, nearly perpendicular to the line joining (2) and (4). The exact angle between these diagonals is ascertained by a method to be given later. The circular casting upon which these posts are mounted may be rotated by means of a tangent-screw with respect to the carriage *C*. Within it a second circular casting is placed, also provided with a tangent-screw, and it is to this inner circle that the photographic plate is clamped.

This device is used as follows: by means of the outer tangent-screw, the line joining (1) and (3) is first made approximately parallel to the guiding way *VV*. Then by rotating the inner circle with its tangent-screw, the plate is oriented in the direction in which *X* co-ordinates are to be measured. The amount by which the line (1) to (3) fails of parallelism with the guiding way is now measured by setting upon (1), moving the carriage along the way until (3) comes into view, and setting upon it, without having disturbed the microscope between the two settings. The difference between these two readings is recorded, and we proceed to measure

[1] I learn that a coarse graduated circle of diameter 30 cm, reading to 0°.05 by a vernier, has recently been added to the machine, particularly for measuring position-angles of double stars and proper-motion stars.

X co-ordinates. The *outer circle* is then unclamped and rotated until the line joining (2) and (4) is nearly parallel to VV. Readings are taken as before upon these dots: if the difference is equal to that for (1) and (3), and if the lines joining the opposite dots are accurately perpendicular, it is evident that we have turned the photograph exactly 90°; it is equally evident that, in any case, we have all the data necessary to say how much the angle differs from 90°, and to apply to the measures the small corrections that are appropriate.

As compared with the usual method, in which a graduated circle is employed, this device has the disadvantage of not permitting the measurement of position-angles. It has the advantages of simplicity, cheapness, and, above all, greater accuracy. For with a graduated circle we have the additional uncertainty due to division errors, which it would be very laborious to determine with sufficient accuracy; whereas with the four posts, we have in effect to determine only one such error, and this we may do at frequent intervals, as the necessary measurements require little time.

We now consider in order the determination and elimination of the various errors to which measures with this engine are liable.

Deviations of the guiding way from perfect straightness.—A long hair was stretched across a brass frame, and the latter clamped in the measuring engine in the position ordinarily occupied by the plate. It was then rotated to make it nearly parallel to the guiding way, and readings upon its center were made at intervals of about 10 mm throughout its entire length. The frame was then rotated 180° around the axis of the hair, and bisections were again made at exactly the same points as before. The means of each pair of these measurements are free from any sinuosities or irregularities in the hair, and would all be equal to each other if the hair were parallel to the guiding way and if the latter were exactly straight. The application of appropriate linear terms therefore gives the deviations of the way.

This procedure was varied by graving a fine line in the film of a photographic plate and measuring it in a similar way, the plate being turned around the line and the reverse measurements being made through the glass. The two determinations gave very

accordant results. These experiments, made in December 1903, were repeated after an interval of fifteen months in March 1905, to ascertain whether the way had suffered appreciable wear. As may be seen from the table the two sets of corrections are practically

TABLE I

CORRECTIONS IN QUARTER-MILLIMETERS FOR DEVIATIONS FROM STRAIGHTNESS OF THE GUIDING WAY

Scale	Corrections December 1903	Corrections March 1905	Adopted Means
mm			
2.....	——	+0.032	+0.030
10.....	+0.023	+ 23	+ 23
20.....	+ 17	+ 18	+ 18
30.....	+ 11	+ 11	+ 11
40.....	+ 7	+ 6	+ 7
50.....	+ 2	+ 2	0
60.....	− 1	− 4	0
70.....	− 3	− 6	0
80.....	− 3	− 5	0
90.....	− 3	——	0
100.....	− 2	− 5	0
110.....	− 2	− 6	0
120.....	− 1	− 5	0
130.....	0	0	0
140.....	+ 2	0	0
150.....	+ 4	+ 1	0
160.....	+ 4	0	0
170.....	+ 4	+ 2	0
180.....	+ 4	+ 5	0
190.....	+ 4	+ 7	0
200.....	+ 2	+ 6	0
210.....	+ 1	+ 2	0
220.....	− 1	+ 6	0
230.....	− 2	+ 4	0
236.....	− 4	+ 1	0

the same. Those determined at the earlier date were adopted as definitive and applied, with the signs shown, to all the measures. The millimeter scale referred to is mounted near the right-hand edge of the carriage *C*, and is parallel to the guiding way. The corrections as actually determined are small for scale-readings 50 to 236 and they were assumed to be zero; the errors that this assumption entails in the position deduced for the parallax star from one plate do not reach 0″.01 for any region in the present list. As a matter of

fact no appreciable error would have been incurred had we neglected the deviations of the guiding way altogether, as each plate is put into the machine in a standard position, and nearly the same portion of the way always comes opposite any one star in a region. So long therefore as the deviations from straightness are not erratic, they are automatically eliminated from the relative positions of the parallax star, when we compare one plate with another. The same remark applies with equal force to the other instrumental corrections, except the "runs" of the micrometer screw, which may vary from day to day.

Division errors of the scale.—Star images can best be bisected with a single micrometer wire, while fine graduations upon a scale are better measured with two parallel wires close together. To retain the advantages of both, the graduations were made double, with components separated by about 0.12 mm. The micrometer wire used was a coarse one (as it had to be with an eyepiece of so low a power) and it nearly filled the space between the two components of each line, leaving two narrow strips of the bright German silver on either side; these two strips could be made very exactly equal and enabled the settings to be made with great accuracy.

The calibration of the scale, that is, the determination of the errors of its 502 lines, was very kindly undertaken for us at the Bureau of Standards through the courtesy of Director Stratton, who thus describes the method employed:

> First, every tenth of the total length was determined by Hansen's method (*Travaux et Mémoires du Bureau International des Poids et Mesures*, Vol. 5), thus giving every 25 mm space, or according to the numbering, the points 100, 200, 300, etc. As a check, the bar was also calibrated into 200 mm spaces by Hansen's method and the values found for the 200 points were combined with the values found for these points by the previous method. Each 25 mm space, or, according to the numbering, each 100 space, was again calibrated by Hansen's method into 5 mm spaces. The separate millimeters of each 5 mm space were then measured with the micrometer screw. All the work referred to above applies to the main or unprimed lines. The positions of the primed lines were next determined by measuring with the micrometer the distance from the main to the primed lines. The micrometer screw used in this work has a value of 43.95 μ per revolution. The total length of the scale was determined by comparison with a calibrated Geneva Society scale whose total length is known in terms of the international meter. The probable errors are

only given for every 5 mm space, but they may be readily computed for other lines by combining the probable error $\pm 0.3\,\mu$[1] (the average probable error of the intermediate spaces) with the probable error of the 5 mm lines nearest to them. This refers to the main or unprimed lines. The probable error of the spaces between each pair of lines is $\pm 0.2\,\mu$ and must be considered in computing the probable errors of any space defined by a primed line. For example the probable error of the space 0 to 72′ is

$$\sqrt{(0.2)^2+(0.3)^2+(0.2)^2} = \pm 0.4\,\mu.$$

In some cases the probable errors will exceed the limit of $0.5\,\mu$, but the lines of the scale are not sufficiently good to make it possible to keep the probable errors below in every case.

The following is a specimen of the form in which the Bureau communicated the errors thus determined:

Graduation	Measured Distance from Zero mm	mm
0	0	
0 to 0′	0	+0.1102
0 to 4	1	− .0035
0 to 4′	1	+ .1097
0 to 8	2	− .0051
0 to 8′	2	+ .1088
0 to 12	3	− .0081
0 to 12′	3	+ .1078
etc.	etc.	

These are the absolute errors of each graduation; but in work like the present only the relative errors are important, and we are therefore at liberty to apply any linear terms that we please. We have accordingly taken the means of the errors for the two components of each line, and then computed

$$4(mean - 0.00007\,n - 0.029),$$

n being the number with which the line is graduated. The factor 4 serves to reduce the corrections to quarter-millimeters. These are the errors or corrections[2] that appear in Table II and they have been applied to all the measures. It will be noticed that they are all positive up to line 616 and all negative thereafter. This circumstance (with which in view these particular linear terms were

[1] This symbol denotes the *micron*, or 0.001 mm.

[2] It is ordinarily the case that errors and corrections have opposite signs; but division errors of a scale are an exception to this rule.

added to the Bureau's corrections) makes their application a little more convenient and lessens the chance of error.

TABLE II

DIVISION ERRORS OF THE SCALE, IN QUARTER-MILLIMETERS

Line	Correction	Line	Correction	Line	Correction	Line	Correction	Line	Correction	Line	Correction
0	+.104	168	+.010	336	+.020	504	+.023	672	−.022	840	−.074
4	+.095	172	+ 8	340	+ 16	508	+ 22	676	− 26	844	− 80
8	+ 89	176	+ 10	344	+ 18	512	+ 26	680	− 26	848	− 83
12	+ 80	180	+ 10	348	+ 25	516	+ 20	684	− 24	852	− 84
16	+ 72	184	+ 12	352	+ 25	520	+ 20	688	− 30	856	− 85
20	+ 69	188	+ 10	356	+ 24	524	+ 23	692	− 30	860	− 82
24	+ 62	192	+ 9	360	+ 22	528	+ 21	696	− 29	864	− 85
28	+ 62	196	+ 10	364	+ 20	532	+ 18	700	− 29	868	− 87
32	+ 58	200	+ 6	368	+ 25	536	+ 18	704	− 32	872	− 85
36	+ 56	204	+ 6	372	+ 27	540	+ 16	708	− 36	876	− 89
40	+ 49	208	+ 4	376	+ 25	544	+ 10	712	− 36	880	− 90
44	+ 46	212	+ 6	380	+ 27	548	+ 5	716	− 38	884	− 96
48	+ 40	216	+ 6	384	+ 20	552	+ 11	720	− 41	888	− 97
52	+ 35	220	+ 8	388	+ 22	556	+ 10	724	− 42	892	− 97
56	+ 36	224	+ 10	392	+ 23	560	+ 6	728	− 44	896	−.104
60	+ 33	228	+ 9	396	+ 28	564	+ 5	732	− 46	900	−.104
64	+ 26	232	+ 10	400	+ 24	568	+ 10	736	− 47	904	−.103
68	+ 19	236	+ 10	404	+ 25	572	+ 15	740	− 44	908	−.104
72	+ 14	240	+ 6	408	+ 23	576	+ 10	744	− 49	912	−.102
76	+ 14	244	+ 3	412	+ 20	580	+ 10	748	− 46	916	−.107
80	+ 15	248	+ 3	416	+ 20	584	+ 11	752	− 50	920	−.108
84	+ 18	252	+ 6	420	+ 20	588	+ 12	756	− 52	924	−.112
88	+ 16	256	+ 5	424	+ 20	592	+ 12	760	− 54	928	−.112
92	+ 16	260	+ 6	428	+ 19	596	+ 8	764	− 50	932	−.114
96	+ 20	264	+ 10	432	+ 23	600	+ 7	768	− 53	936	−.118
100	+ 18	268	+ 5	436	+ 23	604	+ 6	772	− 58	940	−.127
104	+ 20	272	+ 4	440	+ 19	608	+ 4	776	− 56	944	−.131
108	+ 16	276	+ 4	444	+ 19	612	+ 2	780	− 60	948	−.129
112	+ 14	280	0	448	+ 14	616	+ 1	784	− 64	952	−.130
116	+ 14	284	+ 5	452	+ 16	620	− 1	788	− 59	956	−.130
120	+ 16	288	+ 2	456	+ 20	624	− 6	792	− 62	960	−.129
124	+ 14	292	+ 6	460	+ 23	628	− 5	796	− 63	964	−.133
128	+ 16	296	+ 5	464	+ 18	632	− 8	800	− 64	968	−.138
132	+ 17	300	+ 10	468	+ 18	636	− 8	804	− 66	972	−.142
136	+ 16	304	+ 4	472	+ 22	640	− 9	808	− 69	976	−.134
140	+ 14	308	+ 6	476	+ 20	644	− 12	812	− 65	980	−.130
144	+ 9	312	+ 5	480	+ 18	648	− 12	816	− 66	984	−.133
148	+ 11	316	+ 6	484	+ 21	652	− 13	820	− 65	988	−.131
152	+ 14	320	+ 7	488	+ 17	656	− 19	824	− 68	992	−.122
156	+ 10	324	+ 12	492	+ 18	660	− 21	828	− 66	996	−.121
160	+ 14	328	+ 14	496	+ 20	664	− 23	832	− 71	1000	−.124
164	+ 11	332	+ 13	500	+ 23	668	− 23	836	− 73		

The division errors are large, and in this respect the scale is a poor one. But the errors have the merit of running very smoothly; thus in only four cases does the error on any line differ by more than one micron from the mean of the two adjacent lines, the greatest

difference being one and a half microns. Probably even these small differences are for the most part apparent only, since the probable error of one such deviation is nearly four-tenths of a micron.

Correction for runs.—The amount by which four turns of the micrometer screw failed of exact equality with one space on the scale was determined once or twice each day, and the corresponding corrections applied to all the measures of that day. In order that the runs should be the same for all parts of the scale, it is necessary that the latter should be equally distant from the micrometer objective in all its positions. A change in this distance of one-twentieth of a millimeter is appreciable in the runs, but cannot be detected (in an optical system like the present one) by the lack of exact focusing or the presence of "parallax." At the outset of the work,[1] the runs were accordingly determined at several places along the scale. Pieces of very thin paper were inserted between the scale and the casting to which it is screwed, until the runs came out the same for all its parts. Due allowance should have been made in this connection for division errors, but the latter were not forthcoming at the time this adjustment was made. Fortunately the error from this source proves to be negligible, thanks to the smoothness with which the division errors run.

Collimation of the micrometer microscope.—For present purposes the line of collimation of the microscope is defined as joining the optical center of the objective with the zero of the micrometer screw. Bisections upon star images were always made with the micrometer set near zero. Ordinary glass was employed in the photographic plates, and consequently the film does not lie in one plane. For this reason it is necessary that the line of collimation should be closely perpendicular to the general plane of the plate, in at least the right-and-left direction. Otherwise the stellar image will be projected upon the scale in different places, depending upon whether the image is above or below the general plane of the plate. Suppose, for example, that the microscope leans toward the right by the

[1] Of the plates here discussed, eighteen were measured before the necessity for this precaution occurred to me. For these, a small additional correction for runs was applied, depending upon what part of the scale had been used.

angle a; then the measure upon a point that is too high by a distance h will be too great by $h \cdot \tan a$, the zero of the scale being toward the left. As h may easily be a quarter of a millimeter, it follows that a must be made less than $0^{\circ}.3$, if we wish to avoid errors as great as one micron, which corresponds to $0''.01$. No means were provided in the design of the measuring engine for actually tilting the microscope, nor would it have been advisable to do so. But the position of the micrometer head may be changed with reference to the screw itself, so that the zero point can be brought to that part of the micrometer screw that gives a line of collimation in the desired position.

To effect this adjustment we measured the distance (M_1) between two points known to be at somewhat different heights, h_1 and h_2. The plate was then reversed by turning it 180° in its own plane, and again we measured the distance (M_2). If we call a_1 the angle between the line of collimation and a plane perpendicular to the plate and the scale we have

$$s+(h_2-h_1)\tan a_1=M_1,$$
$$s-(h_2-h_1)\tan a_1=M_2,$$

where s is the true distance between the two points. The zero of the micrometer was now changed by three turns of the screw and the distance measured a third and a fourth time:

$$s+(h_2-h_1)\tan a_2=M_3,$$
$$s-(h_2-h_1)\tan a_2=M_4.$$

We now have all the data necessary to determine a_1 and a_2 and therefore to state where the zero of the micrometer should be taken.

Periodic and progressive errors of the errors of the micrometer screw.—An auxiliary micrometer was mounted upon the carriage with its wire in the plane ordinarily occupied by the photographic film and with the screw parallel to that of our own micrometer. The method of Gill and Lorentzen (described by Jacoby in the *American Journal of Science*, [4] **1**, 333, May 1896) was employed, yielding the errors of the two screws. Both the periodic and the progressive errors of our screw proved to be entirely negligible.

Determination of the angle between the posts.—In the quadrilateral formed by the dots we may readily measure the four sides,

a_1, a_2, a_3 and a_4; a_1 being between (1) and (2), a_2 between (2) and (3), etc. Theoretically we need another dimension (for example, the angle at one of the vertices or the length of a diagonal) in order to compute the angle between the diagonals. But in practice it comes out that we do not have to know any dimension beyond those of the sides. For, if $90°+\theta$ is the angle between the diagonals on the side toward (1) and (4), and $90°+\delta_1$, $90°+\delta_2$, etc., the angles at the vertices, then we have

$$\tfrac{1}{2}\tan\theta=\frac{-a_1+a_2-a_3+a_4}{a_1+a_2+a_3+a_4},$$

in which we have neglected only second order and higher terms in δ_1, δ_2, etc., and similar terms in the differences between the sides. So long therefore as the quadrilateral does not differ too much from a square, we may compute θ from this expression with sufficient accuracy.

Direct measurement of the two diagonals enabled us to compute within a minute of arc the angles δ_1, δ_2, etc., and thus to prove that the sides of the quadrilateral are all parallel or perpendicular to each other within $6'$. With practically no error we have then the following co-ordinates of the four dots referred to (1) as origin and (a_4) as the axis of X:

$$(1)\begin{cases}0\\0\end{cases}\qquad(2)\begin{cases}-a_1\cdot\sin\delta_1\\a_1\end{cases}\qquad(3)\begin{cases}a_2-a_1\cdot\sin\delta_1\\a_3\end{cases}\qquad(4)\begin{cases}a_4\\0\end{cases}$$

Consequently

$$\tan(314)=\frac{a_3}{a_2-a_1\cdot\sin\delta_1},\quad\text{and}\quad\tan(241)=\frac{a_1}{a_4+a_1\cdot\sin\delta_1}$$

$$\tan\theta=\operatorname{cotan}\{(314)+(241)\}=$$

$$\frac{a_2\cdot a_4-a_1\cdot a_3+a_1\cdot a_2\cdot\sin\delta_1-a_1\cdot a_4\cdot\sin\delta_1-a_1^2\cdot\sin^2\delta_1}{a_1\cdot a_2+a_3\cdot a_4-a_1^2\cdot\sin\delta_1+a_1\cdot a_3\cdot\sin\delta_1}.$$

Let us replace a_1 by $M+\gamma_1$, a_2 by $M+\gamma_2$, etc., M being the mean of the four sides. In the present case none of the quantities γ exceeds $0.0007\,M$, and we may therefore neglect terms of the order of γ^2/M^2. Similarly terms in δ_1^2 may be neglected, δ_1 being less than $3'$. Consequently

$$\tan\theta=\frac{-\gamma_1+\gamma_2-\gamma_3+\gamma_4}{2\,M},\quad\text{or}\quad\tfrac{1}{2}\tan\theta=\frac{-a_1+a_2-a_3+a_4}{a_1+a_2+a_3+a_4}.$$

This expression yields a value of θ that is correct within less than 1″. A rotation of this amount corresponds to only 0″.01 in an arc of 35′ upon the plates.

An investigation of the errors of the microscope way, that is, the one upon which the microscope travels right and left, is hardly necessary in the present connection. Its deviations from straightness would have to be large in order to change the orientation of the axis A enough to make an appreciable difference in the projected position of an image, the distance between the image and the scale being small. Furthermore, any error of this kind would be eliminated by the insertion in the engine in a standard position of all the plates of one region.

The orientation of the first plate of a region was effected through the aid of catalogue-positions of the parallax star and one or two of the comparison stars; or, more commonly, by means of a fourth image, or *trail*, of the parallax star. This orienting image was secured by unclamping the driving clock, and then, after the lapse of perhaps a minute, allowing it to drive long enough to secure a measurable image of the parallax star. Corrections for curvature[1] of path were applied whenever necessary, but no time was wasted in attempting to orient the first plate of a region (closer than a few minutes of arc) to the exact direction of the equator or the ecliptic. It is obvious that unless the parallax were very large, no material error would be incurred if all the plates were measured as much as half a degree from the direction for which the parallax factors are computed. It is, however, of some importance that the second and succeeding plates should be oriented just like the first. To do this we rotated the plate until the difference between the co-ordinates of any two comparison stars came out substantially the same as in the first instance. The plate was then moved to the right or the left so as to bring the middle image of the parallax star opposite a predetermined division on the scale. We might have adjusted the plates so as to bring the *comparison* stars always opposite standard places on the scale, which is not quite the same thing, owing to the large proper motions of some of the parallax stars here included. But the alternative actually adopted is a little preferable, since

[1] *Contributions from the Observatory of Columbia University*, No. 16, p. 62.

outstanding uncertainties in the division-errors, in the screw-errors, etc., are thus more likely to be accidental instead of systematic in their effect upon the relative position of the parallax star.

The images of the parallax star were measured twice and those of the comparison stars, of which there were ordinarily about five, usually only once. But in a few cases where, owing to its position, one of the comparison stars assumed unusual importance,[1] its images were likewise measured twice.

Up to March 1905, nearly every plate was measured both by Miss Ware and myself, but preliminary reductions showed that this was unnecessary and uneconomical. In other words we found (and this seems to be the case with all photographic work) that the mere errors of bisection are small as compared with those that are inherent in the plate itself, or possibly, in the case of photographs taken with smaller telescopes, those that have their origin in the measuring engine. After March 1905, all the measurements were accordingly made by Miss Ware alone. Another reason for coming to this decision was that, owing to my removing from Williams Bay at this date, it would have been very inconvenient, though not prohibitively so, for me to have continued with the work of measuring.

While no systematic difference between the two observers came to light, care was taken to combine the two sets of measures in such a way as to eliminate, in every case, the effect of a possible difference of this sort upon the deduced parallax.

This portion of our subject may properly be terminated with a brief reference to other methods for securing and measuring the plates, with a statement of the reasons that have led me to prefer those just described.

The following procedure was suggested many years ago by Kapteyn and has since been put into practice by him and others: A plate is exposed to the desired region and is then stored without development for half a year. A second exposure is now secured one millimeter to the right of the first and the plate is stored for an additional half-year; then a third and final impression one millimeter to the right of the second is obtained, and the plate is

[1] An exact criterion for the importance of a comparison star will be given later.

developed. From the relative positions of the three images of each star we may deduce parallaxes and proper motions. Assuming that precautions have been taken against hour-angle error and optical distortion, the only known sources of error that remain are those due to uncertainties in the bisections and the guiding error.

After giving the matter some thought, I decided that the advantages of this method, great though they may be in some cases, are outweighed by its disadvantages under the conditions that actually confronted me. The first of these is the additional time consumed at the telescope. A particular plate must be selected without chance of mistake, from perhaps one hundred that are stored in a dark room; the plate must be inserted in its holder, and the holder in the carrier at the telescope, in standard positions. Compare this with simply taking any plate from a box of fresh ones and inserting it in the holder in whatever position it chances to assume. With Kapteyn's method the holder, the carrier, and the plate itself must be such that the second and third exposures shall be oriented very closely the same as the first. Otherwise the relative distances between the three images will change from star to star and uncertainties in the screw-errors, etc., may affect the deduced parallaxes. The relative positions of the three images should be the same within 0.1 mm, or even 0.05 mm in some cases. While there is no real difficulty in doing this, it must always involve a loss of valuable time.

With a telescope of so great a focal length as the 40-inch there is considerable liability of failure for some of the exposures. When the seeing is poor, and to a less extent when the atmosphere is only imperfectly transparent, it is a matter of difficulty to gauge the length of exposures required. It was my practice, whenever circumstances called for it, to develop the first two or three plates, and to expose the following plates accordingly. This cannot be done with Kapteyn's method unless the observer makes exposures for this exclusive purpose.

Even if the exposure is not a failure outright, but is, let us say, entitled to half weight, it can be shown that Kapteyn's method always involves a loss of weight for the deduced parallaxes. For illustration, suppose that we have two Kapteyn plates upon each of which two exposures have been impressed at the middle epoch.

On the first plate suppose that these *middle* exposures have half weight, and that on the second plate the exposures at the *first* and the *third* epochs have half weight. Then if the parallax factors are all ± 1 and if the intervals are just half a year, each plate will yield 2.67 as the weight for the parallax. If now the plates are measured in the ordinary way and combined into one solution, the parallax comes out with a weight of 6.00, which is considerably greater than the sum of the Kapteyn weights. This example is by no means an extreme case; we shall see later that some of the plates here discussed are entitled to several times the weight that can properly be assigned to others.

With Kapteyn's method the observer must incur one of two other disadvantages. If he elects to measure in right-ascension there is a further loss of weight for the deduced parallax as compared with measuring in longitude. If, on the other hand, he decides to measure in the latter direction he must set his plate-carrier at the telescope accordingly; for, since the parallax is deduced from measurements of the *distances* between the images, it is necessary that the plate should be shifted, from one exposure to the next, in the direction for which the parallax factors are to be computed. In either case there is not the same freedom at the telescope as with the ordinary method, and the observer cannot revolve his plate-carrier into any position that may aid him to reach a suitable guiding star or to secure a better distribution of comparison stars. This advantage might be retained, however, by providing a third set of slides in the construction of the plate-carrier.

In point of accuracy the chief difference between the two methods is that one of them eliminates distortions of the film and therefore yields a somewhat smaller probable error for a plate. It is therefore necessary to know the average size of these distortions in order to make a choice between these methods in any particular case. When this work was begun, astronomers were in general of the opinion (that has since proved to be abundantly justified) that the distortions are very small. But shortly afterward some doubts upon this matter were expressed, and I decided to investigate it for the same plates and other conditions that apply to the

work itself. In this investigation, begun at the Yerkes Observatory, and completed at Allegheny,[1] an effort was made to separate this error from all others. The outcome was to show that the mean value of the distortions (in a sense analogous to mean error) is of the order of one micron. This corresponds to a probable error of only 0".007 on these plates. The mean of the three exposures on a plate is probably affected by an even smaller quantity, as the images of a star extend over ten millimeters, and it is not likely that the distortion persists, both as to amount and direction, over such distances. But waiving this point, this source of error can in any case form only a small fraction of the total plate-error. In the most favorable case, a plate containing three good exposures that have been measured by two observers has a probable error of 0".020, which would be reduced by at most 0".0013 if the distortion of the film had been entirely eliminated. The reduction is of course less if the plates are not so good, that is, if their probable error is greater.

By storing the plates in a dark room for long intervals of time, the observer keeps himself in the dark as well. He does not know how his work at the telescope is coming out and he is not free to modify in any way the conditions with which he set out, as for example the length of exposure, the choice of a guiding star, etc. Nor can he begin to measure his plates until a year has elapsed—an important consideration in the present case, where the time available for the whole research was limited.

The use of a *réseau*, like those employed in connection with the *Astrographic Catalogue*, was also carefully considered, but it was rejected for reasons that are very much the same as some of those just explained. The *réseau* would hamper the work at this telescope, though not to the same extent as with Kapteyn's method. It is doubtful even now whether plates measured with or without the *réseau* are the more accurate, and its use would not simplify the work of measuring where only a few images are to be located on each plate.

The reader will understand that I have criticized these methods from the single point of view that was forced upon me by the circumstances with which I had to deal. Some of the objections to

[1] *Publications of the Allegheny Observatory*, 1, 1, 1907.

which I have called attention are altogether inapplicable to other cases. For example, with smaller telescopes the guiding is usually not done with a double-slide plate-carrier but by following the field with an auxiliary telescope and moving the two tubes as a unit. Again, distortions of the film and instrumental errors are small in the present work, the scale of the plates being so large, and the guiding error is the largest item of inaccuracy. But with shorter focal lengths these sources of error are absolutely and relatively more important, and may in some cases equal the guiding error or at least become more nearly comparable with it. The observer must decide from the circumstances in each case how to expend the time and energy at his disposal, so as to secure the most accurate results for the work in hand. Just where the various methods cease to be advantageous I do not presume to say, but I may venture the opinion that, on the one hand, the dividing line will be found close to work of the character to which Kapteyn has applied his method; that is, the derivation of the mean parallaxes of a large number of stars within a restricted area. On the other hand, it seems to me that the usefulness of the *réseau* is nearly limited to work like that of the *Astrographic Catalogue*, where the object is to measure as rapidly as possible both co-ordinates of numerous star images.

ALLEGHENY OBSERVATORY
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THE PRODUCTION OF LIGHT BY CANAL RAYS

By GORDON SCOTT FULCHER

The two phenomena of greatest significance in connection with canal rays are well known to be the simultaneous electrostatic and magnetic deflection of the rays, first obtained by W. Wien, and the Doppler effect, which J. Stark discovered to be shown by light from the path of the canal rays. The Wien experiment enables us to determine the velocity and mean value of $\frac{e}{m}$ of the constituent rays; the Stark effect gives us directly the speed in the line of sight of the sources of the light showing the effect. Two years ago, from a summary and comparison of the experimental results reported by W. Wien, J. J. Thomson, J. Stark, F. Paschen, and others,[1] it seemed evident that the distribution of velocities among the canal rays is quite different from that among the sources of the light showing the Stark effect. It appeared to be necessary to conclude that the canal rays cannot themselves be the chief sources of the light in question, as has been generally assumed.[2]

THE HYPOTHESIS

An alternative hypothesis was suggested. The only other molecules present with velocities great enough for them to be the sources sought are those gas molecules which have been hit by the canal rays and thus have acquired a velocity great or small according to the squareness of the collision. If we suppose this molecular phenomenon to obey the laws of ordinary impact between solid bodies, the momentum given the gas molecule will vary up to a maximum value of the same order as that of the bombarding ray, depending on the relative masses of the two molecules and on the coefficient of restitution. Will the Doppler effect to be expected if these hit molecules emit light agree in all essential

[1] G. S. Fulcher, "Our Present Knowledge of Canal Rays," *Smithsonian Misc. Collections*, **52**, 295–324, 1909.

[2] *Ibid.*, p. 322.

details with the Stark effect as observed? A statistical computation, explained in detail below (p. 41), showed that a satisfactory agreement was obtained if the following assumptions were made:

1. That the intensity of the light emitted as a result of each impact is proportional to the energy transmitted to the hit molecule, but

2. That the hit molecule emits no light unless the energy so transmitted exceeds a certain minimum supposed to be equal to that necessary to produce ionization; and

3. That the hitting molecules emit no light of the kind showing the Stark effect as a result of the collision. Of these, the first has since been experimentally verified (see below), the second is rendered probable by other experimental evidence, and the third, rather more difficult to accept, will be shown later to be a necessary assumption which perhaps can be made more plausible by the consideration that most of the hitting charged rays may be neutralized by the collisions which ionize the hit molecules. Hence if the hitting molecules thus neutralized emit any light, it is not the series-line spectrum which is emitted by the ionized molecules and which alone shows the Stark effect.

It is possible then to reconcile the results of the deflection experiments with the Stark effect if we assume that the sources of the light showing this effect are the gas molecules hit and ionized by the canal rays rather than the canal rays themselves.[1]

EXPERIMENTAL EVIDENCE

If this hypothesis is correct the intensity of the light from a canal-ray beam should vary directly as the number of collisions per unit time, per unit length of path, that is, with the pressure of the gas, providing the number and velocity of the canal rays are kept constant.

The following apparatus, shown diagrammatically in Fig. 1, has been designed to test this deduction. It is so arranged that the pressure of the gas in the canal-ray chamber back of the cathode can be varied in the ratio of one to twenty without changing the pressure in the discharge chamber, which determines the

[1] *Ibid.*

cathode-fall of potential, that is, the number and velocity of the canal rays. In one experiment hydrogen gas from the reservoir where the pressure is maintained at a certain constant value of about 5 cm of mercury, passes slowly and continuously through

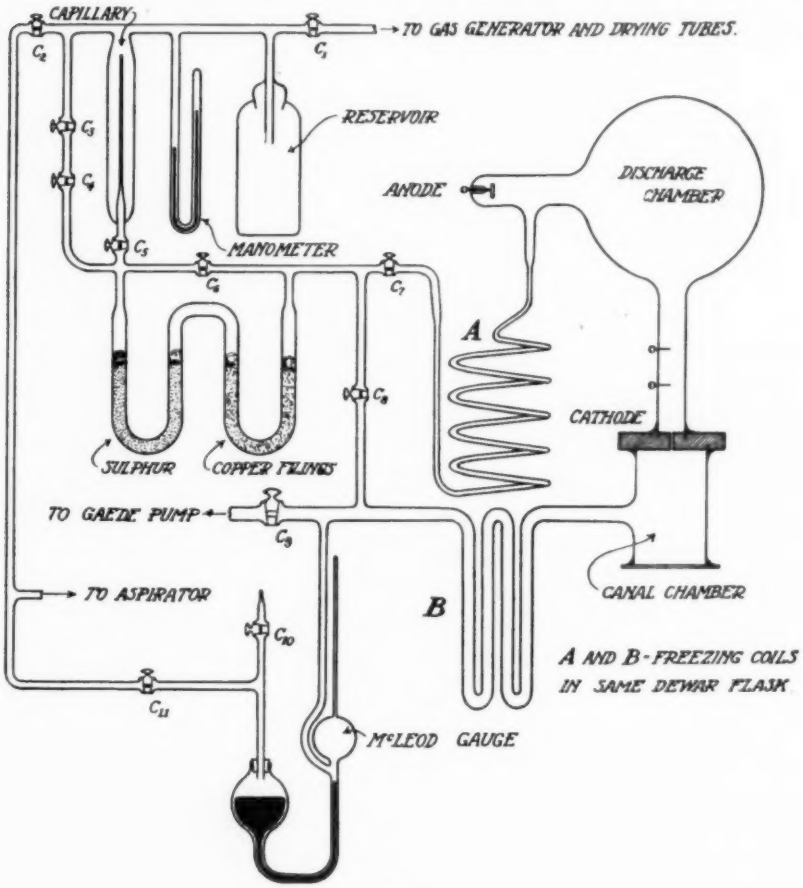

FIG. 1

a capillary tube (10 cm long and with a bore about 0.009 cm in diameter) and some purifying tubes to the discharge chamber. The pressure here is about 0.1 mm of mercury. From this chamber a single hole in the aluminum cathode (0.05 cm in diameter and 1 cm long) leads to the canal chamber, which is

maintained at a pressure of about .005 mm of mercury by a Gaede mercury pump to which it is connected by large tubes. The hole in the cathode thus behaves like a porous plug of a single pore. After equilibrium is established, these pressure relations can be maintained indefinitely, and the intensity of the light emitted when the canal rays pass through gas at the low pressure maintained in the canal chamber can be measured at leisure.[1]

On the other hand the pump may be stopped and the pressure allowed to become the same on both sides of the cathode and equal to the pressure formerly existing only on the discharge-chamber

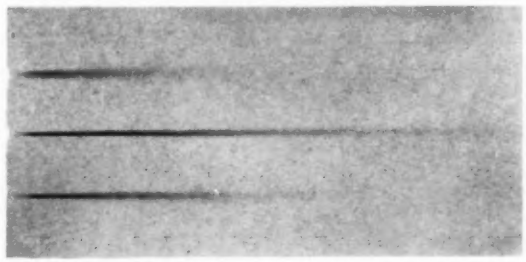

FIG. 2.—Photographs of the canal-ray beam
a, Beam through gas at low pressure (0.005 mm)
b, Beam through gas at higher pressure (0.1 mm)
c, Extra-focal image of beam, higher pressure

side. Conditions in the discharge chamber will now be the same as in the first experiment; hence the current and the cathode-fall of potential, and therefore the number and velocity of the canal rays, will be the same in both cases, though the pressure in the canal chamber is quite different. Hence the intensity of the light from the canal-ray beam should vary in the same ratio as the pressure of the gas in the canal chamber, if our hypothesis is correct.

To measure the intensity of the light, the beam was photographed with an ordinary camera. The time of exposure was adjusted so that in all cases images of about the same density were obtained. The camera was thrown slightly out of focus so as to secure broader images whose density could be more easily compared

[1] To avoid confusion, the discussion of certain details of the apparatus is postponed to the end of the paper.

by the use of a spectrophotometer (see Fig. 2). The intensity of the light was assumed proportional to the density of the image divided by the time of exposure. The pressure was measured with a McLeod gauge connected to the canal chamber. The results for two cathode-falls of potential are given in the following table.

TABLE I

Cathode-Fall Volts		Pressure mm of *Hg*	Time of Exposure *t*	Density of Image *D*	Intensity of Light $I=\frac{D}{t}$	I_1 / I_2	p_1 / p_2	$\frac{I_1}{p_1}$ / $\frac{I_2}{p_2}$
3900	1	0.102	60 sec.	0.41	0.0068	17.5	19.2	0.91
	2	0.0053	1200 "	0.47	0.00039			
4400	1	0.096	50 sec.	0.44	0.0088	22.6	21.9	1.03
	2	0.0044	1200 "	0.47	0.00039			

If the hypothesis is correct $\frac{I_1}{p_1}$ should be approximately equal to $\frac{I_2}{p_2}$. The agreement, though unsatisfactory, is within experimental errors.

It is here assumed that light of a certain intensity acting for twenty minutes will produce the same density of image as light of twenty times the intensity acting for one minute. To test this, half a plate was exposed to light from a point source at a distance of 125 cm for 45 seconds and the other half was exposed for 720 seconds at a distance of 500 cm from the same source. Here the ratio of intensities is 16 and equal to the inverse ratio of the times of exposure. No difference can be observed between the densities of the two halves of the developed plate—proving that for the intensities used the assumption is sufficiently accurate.

I gladly acknowledge my indebtedness to Professor C. E. Mendenhall for help in designing the apparatus. The use of a capillary in combination with a Gaede pump to secure equilibrium conditions and thus make the experiment quantitative was suggested by him.

The conclusion from this experiment is, therefore, that the intensity of the light from the canal-ray beam is proportional to

the pressure of the gas through which the canal rays pass, that is, to the probable number of collisions per unit length of path, per unit time, between the canal rays and gas molecules, provided that the number and velocity of the canal rays is unchanged.[1]

Next the question arises, how does the intensity of the light emitted vary with the velocity of the rays? Does it vary directly as the total energy-flux of the rays times the gas pressure, that is,

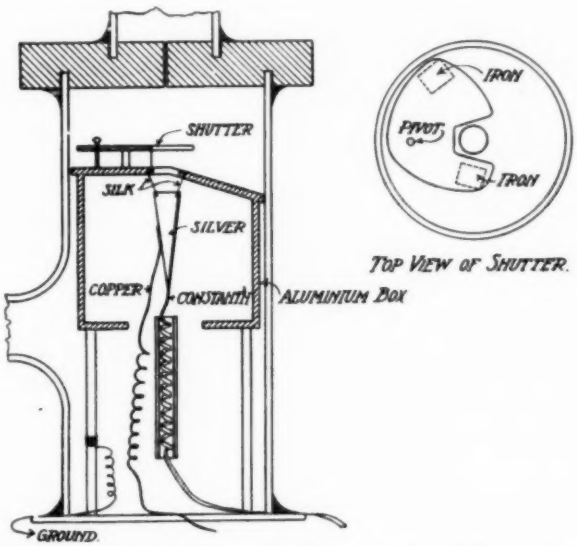

FIG. 3.—Apparatus for measuring the energy-flux of the canal rays

as the mean energy of each ray times the number of collisions per unit length of path per second? To answer this it became necessary to measure the energy-flux of the rays as a function of the cathode-fall of potential. To this end, they were allowed to strike inside a light silver cone so that their kinetic energy might be transformed to heat energy and measured from the temperature changes produced.

In the canal chamber the apparatus shown in Fig. 3 was placed. The silver cone weighing only a fifth of a gram (3 cm long and 0.5 cm in diameter at its base) was suspended in the path of the rays,

[1] This result was reported at a meeting of the Am. Phys. Soc., November 27, 1909.

by means of silk threads, from a grounded aluminum box which served to shield the cone from sudden changes of temperature. To measure the instantaneous temperature of the cone a thermocouple was used. Along one side of the cone was soldered a constantan wire rolled flat, along the other a copper wire. The other junction (see Fig. 4) was maintained at a constant adjustable temperature by immersion in kerosene in a Dewar flask provided

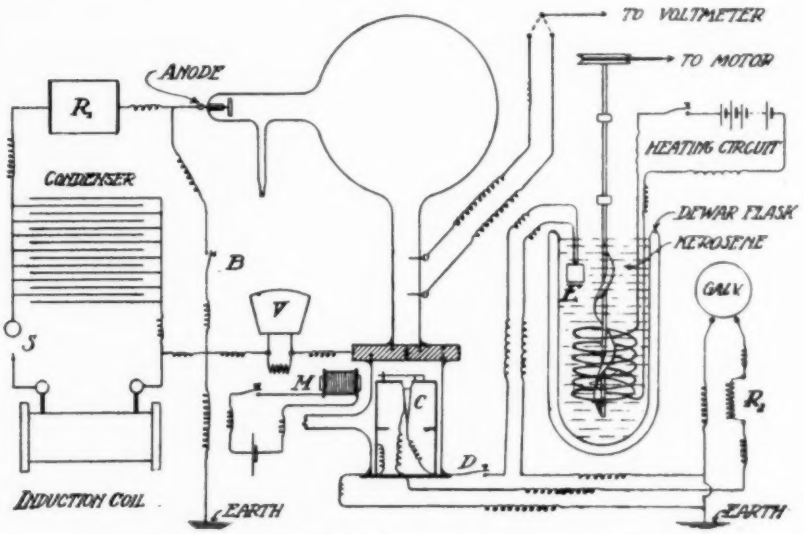

FIG. 4.—Diagram showing the electrical connections of the apparatus

with a heating coil and stirrer.[1] To measure the thermo-electric current a Thomson galvanometer was used whose sensitiveness of 4×10^{-9} amperes per mm deflection could be decreased by introducing resistance R_2. For the most sensitive adjustment used ($R_2=0$), 1 mm deflection corresponded to about 0.001°C increase in temperature of the cone or a net addition to the cone of 1.2×10^{-5} calories. Two telescopes were mounted so that through one the observer could follow the second hand of a watch while noting the galvanometer reading with the other eye. Thus a series of observations at intervals of 5 seconds could be made and

[1] To avoid confusion, the discussion of other details of the apparatus is postponed to the end of the paper.

the instantaneous temperature of the cone as a function of the time determined. Typical curves are shown in Fig. 5. On starting the discharge through the tube the temperature rises rapidly at first, then more and more slowly. The curve never becomes horizontal because of the radiation received from the back surface of the cathode, which increases with the time as the cathode is

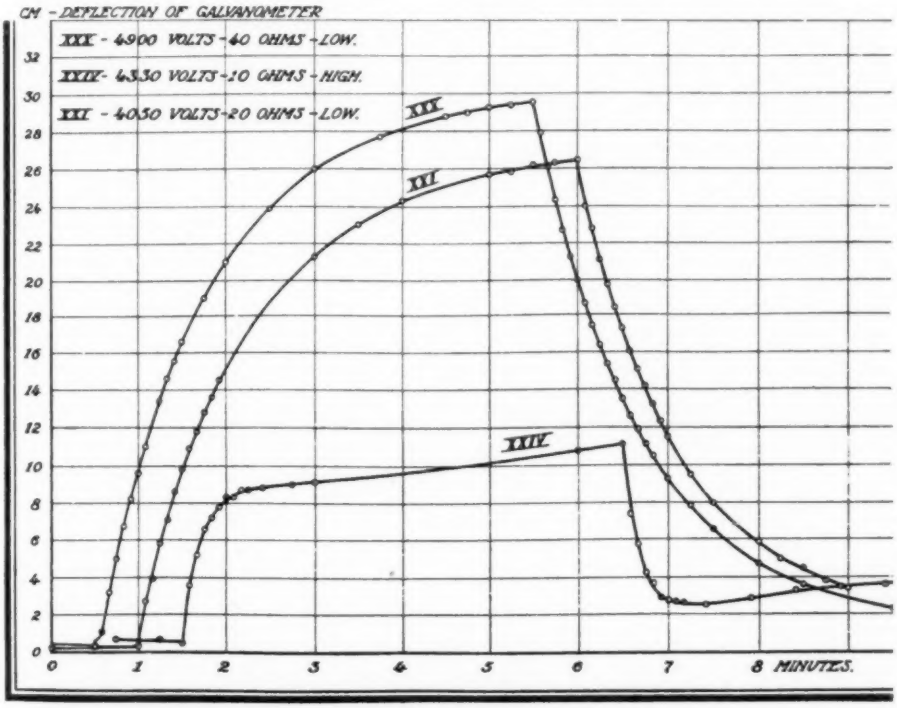

FIG. 5.—Temperature of the cone as a function of the time

heated by the discharge. At the instant when the discharge is stopped, the quickness with which rapid cooling begins is noteworthy. Curves XXI and XXX were taken when the pressure in the canal chamber was low (0.005 mm), while curve XXIV was obtained with a much higher pressure (0.1 mm). The sensitiveness is twice as great for XXIV as for XXI and that in turn twice as great as for XXX.

The problem now is to determine the value of the energy-flux

corresponding to each of these curves. It is easily shown that the temperature is in no case a simple exponential function of the time, as would be the case if the heating of the cone by the rays and the radiation of heat to a constant temperature envelope alone were involved. The radiation from the heated cathode is an important disturbing factor. If we let

x = temperature of the cone,
y = temperature of the surrounding gas and case,
z = temperature of back side of the cathode,
c = heat capacity of the cone,
E = energy received by the cone from the canal rays per second,
A and B = radiation constants, and
C = convection constant depending on the gas pressure,

then

$$c\frac{dx}{dt}=E+A\ (z-x)-(B+C)(x-y) \tag{1}$$

Just preceding the instant ($t=5$) when the discharge is discontinued by short-circuiting the tube we have

$$c\left(\frac{dx}{dt}\right)_{-5}=E+Az_5+(B+C)y_5-(A+B+C)x_5 \tag{2}$$

while immediately following the cessation of the discharge

$$c\left(\frac{dx}{dt}\right)_{+5}=Az_5+(B+C)y_5-(A+B+C)x_5 \tag{3}$$

since there is no discontinuity at that instant of either x, y, or z. Hence

$$E=c\left(\frac{dx}{dt}\right)_{-5}-c\left(\frac{dx}{dt}\right)_{+5} \tag{4}$$

an equation which serves most readily for the determination of E from the curves.

The difficulty then was to determine $\left(\frac{dx}{dt}\right)_{+}$ accurately, since the cooling was so rapid at first that the galvanometer readings were uncertain immediately after the discharge was stopped. The method employed was a graphical one. A smooth curve was drawn through the galvanometer readings plotted as a function of the time (Fig. 5), making due allowance for the fact that the

current carried by the rays, which in part passed through the galvanometer, ceased at the same instant as the discharge. The ordinates of the curve were read at regular intervals of time and the values of the time derivatives thus obtained were plotted as a

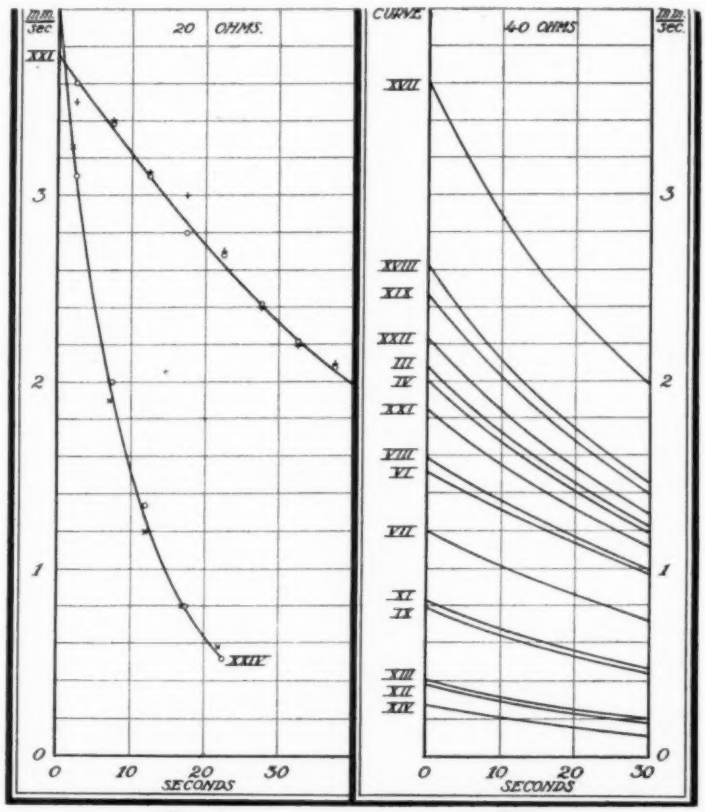

FIG. 6 FIG. 7

Time derivative of the temperature of the cone as a function of the time

function of the time. Fig. 6 shows two such derivative-time curves corresponding to two of the temperature-time curves of Fig. 5. The circles were obtained from the cooling part of the curves, the crosses from the heating part. The former are the more reliable. It is seen that a smooth curve can be drawn through the points with considerable certainty for XXI (low pressure)

and the value of the derivative for $t=0$ can be determined with some accuracy. A number of such derivative-time curves are shown plotted to the same scale in Fig. 7. These curves were each drawn independently. The fact that they fit in with each other so well is evidence for their accuracy.

The results obtained for the energy-flux of the canal rays as a function of the cathode-fall of potential are plotted in Fig. 8. It

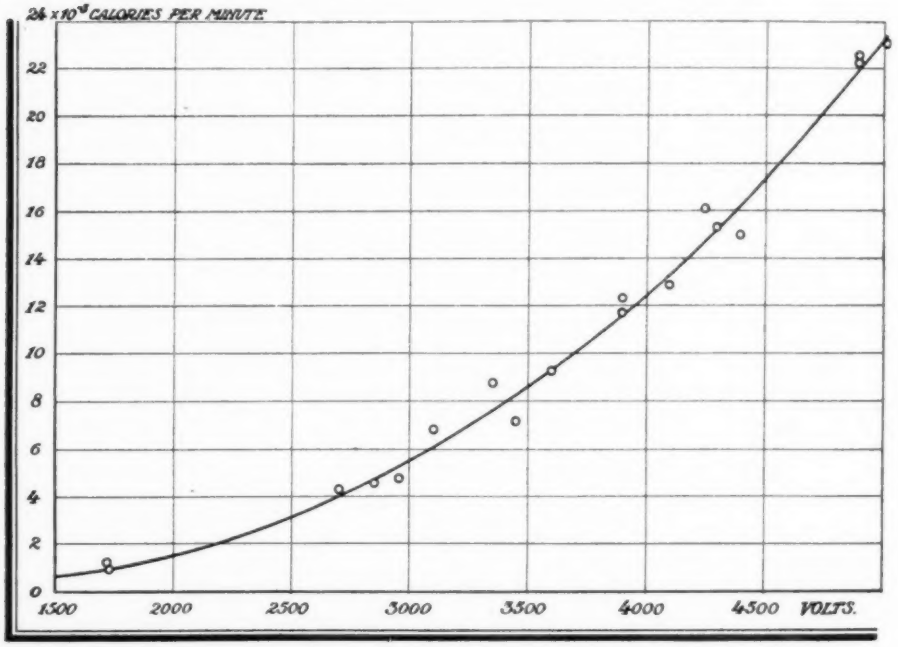

FIG. 8.—Energy-flux of canal-ray bundle as a function of the cathode-fall of potential

is to be regretted that the agreement between the various points obtained is not better. The trouble does not lie in the determination of the energy-flux from the curves, since that is probably accurate to a few per cent, but rather in the measurement of the cathode-fall of potential. The Kelvin electrostatic voltmeter could be read to 1 per cent, but even though special precautions were taken, as will be described later, to secure a continuous discharge through the tube, the cathode-fall of potential was seldom quite constant, so that readings could not be made as accurately as

otherwise. The effect of slight impurities in the gas, such as were doubtless given out by the cathode during the discharge, is very marked, though in no cases were these impurities sufficient to affect the spectrum to any noticeable extent. It is seen that a change of less than 5 per cent in the abscissas of the points plotted in Fig. 8 would bring all of them on the curve drawn through them. We

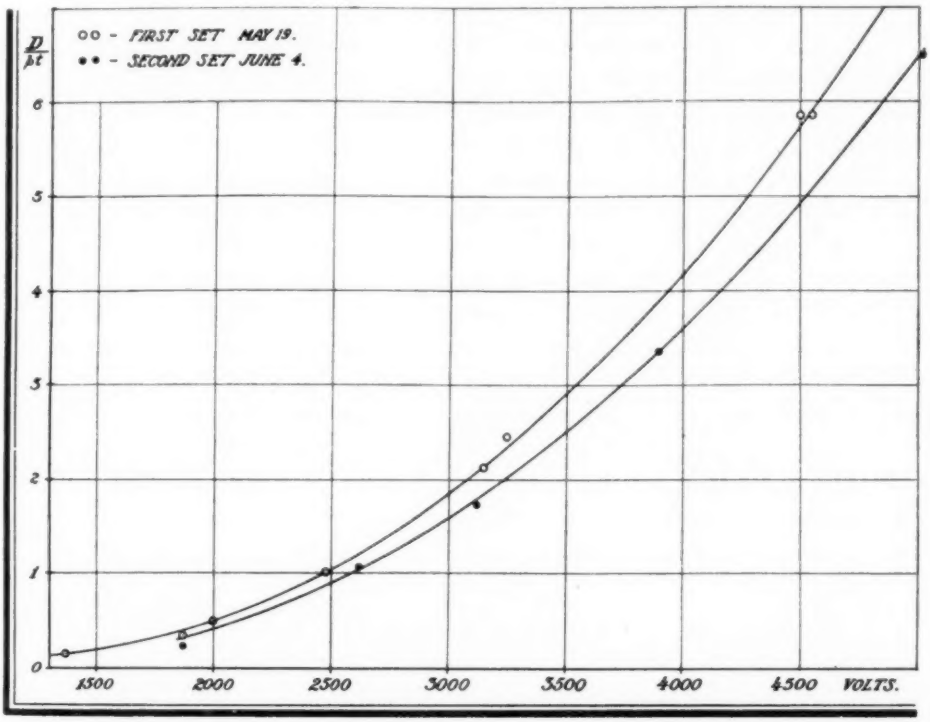

FIG. 9.—Ratio of light-intensity to gas pressure as function of cathode-fall of potential

must conclude, therefore, that the curve represents the experimental results as closely as possible and that the deviations are less than possible experimental errors.

The variation of the intensity of the light from the canal-ray beam as a function of the cathode-fall of potential was determined precisely as in the first experiment by photographing the beam, adjusting the times of exposure so as to obtain images of about the same density. The results are plotted in Fig. 9. The ordinates

are proportional to $\frac{D}{pt}$, where D is the density of the image measured with a spectrophotometer, p is the gas pressure, and t the time of exposure. Two sets of determinations were made. The two curves drawn through the points obtained are each identical with the curve shown in Fig. 8 except for a constant factor of proportionality. The ordinates of Fig. 8 are proportional to the mean energy of the individual rays times the number of rays striking the cone per second when the pressure in the canal chamber is low; that is, they are very closely proportional to the mean energy of the individual rays times the number of rays emerging from the hole in the cathode per second when the pressure is the same on both sides of the cathode. The ordinates of the curves in Fig. 9 are proportional to the mean intensity of the light emitted as a result of each collision times the number of rays emerging from the hole in the cathode per second when the pressure is the same on both sides of the cathode. The fact that through the range of cathode-falls used these curves agree shows that the mean intensity of light emitted per collision is proportional to the mean energy of the individual rays, that is, to the mean energy of each collision. It is not to be expected that this law holds rigorously, and deviations may be expected to increase as the minimum voltage necessary to produce a discharge is approached; but within the limits of voltage and velocity used here (1,500 to 5,000 volts, 6 to $10\times10^7 \frac{\text{cm}}{\text{sec}}$) the law seems to be verified within the limits of experimental error, that is, within a few per cent.

DISCUSSION AND STATISTICAL CALCULATIONS

What bearing have these experimental results upon the theory of the production of the light in question? First, it must be pointed out that in the case of canal rays in pure hydrogen the light producing the displaced lines in the Stark effect is several times as intense as that producing the rest lines, hence the experimental results which apply strictly only to the whole of the light from the path of the rays may be taken without serious error to apply to the light from the moving sources alone. The experi-

ments therefore tend to prove that the light showing the Stark effect is emitted only as a result of the collisions of canal rays with gas molecules and that the intensity of the light emitted per collision is proportional to the mean energy imparted to the hit molecules by the collisions.

Can the details of the Stark effect be explained on this basis? If so, what additional assumptions are necessary? In answering these questions a calculation made three years ago, the results of which were reported in part in the article referred to above, is of some importance. By a statistical method a computation was made of the Stark effect to be expected if the following assumptions are true:

1. That canal rays all with the same velocity enter a gas whose molecules are identical with the rays and have velocities negligible in comparison with that of the rays;

2. That the momenta after collision are the same as if the colliding molecules were perfectly elastic spheres;

3. That the intensity of the light emitted by each hit molecule is proportional to the kinetic energy given it as a result of the collision, but

4. That no light is emitted by a hit molecule unless the energy transmitted to it exceeds a certain minimum, which is supposed to be equal to the energy necessary to produce ionization; and finally

5. That no light of the kind in question is emitted by the hitting rays—the supposition being that they are for the most part neutralized by the collisions, hence do not emit the same spectral lines as the ionized hit molecules.

Of these assumptions the first two are made for the sake of simplicity. Their disagreement with the actual facts in the case of hydrogen rays in hydrogen gas is probably not sufficient to affect the qualitative value of the computation. The third assumption has since been proved by the experiment described above. The last two are equivalent to the single assumption that the series-line spectrum is emitted only by the molecules which become positively charged as a result of the shock of the collision. To find out whether this assumption is necessary to explain the details

of the Stark effect, as reported by J. Stark, F. Paschen, and B. Strasser, on the basis of the emission of light only as a result of molecular collision (necessitated by the first experiment above), is the purpose of the following computations.

The method employed was necessarily statistical, as the problem is too complicated to yield to direct mathematical treatment. Using the second assumption, we can readily determine the velocity

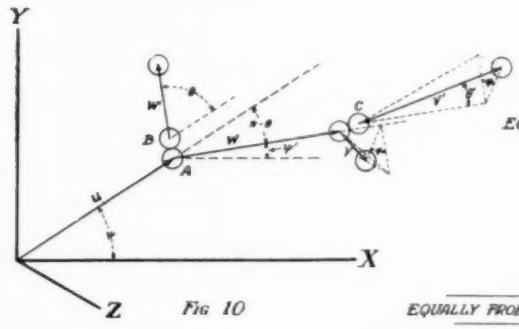

Fig 10

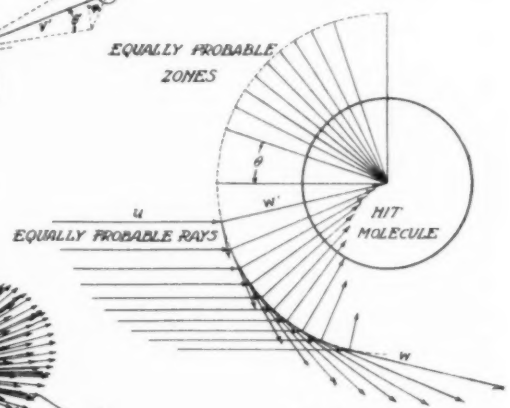

Fig 11

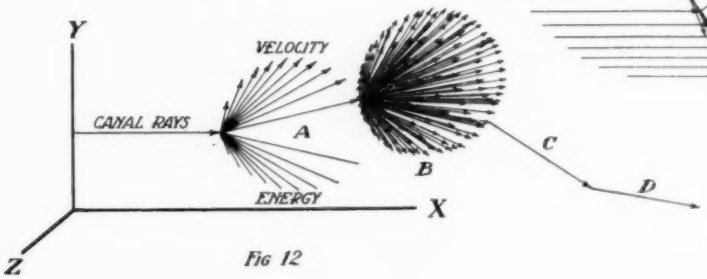

Fig 12

of the hit and of the hitting molecule after each impact. If a molecule with velocity u (Fig. 10) hits a molecule at rest so that the line of centers at the instant of collision lies in the XY plane and makes an angle θ with u, the resulting velocities are:

(for hit molecule) $\quad W' = u \sin\theta\,;\ W'_x = W'(\cos\theta\cos\psi - \sin\theta\sin\psi)$
(for hitting molecule) $\quad W = u\cos\theta\,;\ W_x = W\ (\sin\theta\cos\psi + \cos\theta\sin\psi)$

If the line of centers is not in the XY plane but its projection on a plane perpendicular to the velocity of the hitting molecule before collision (W) makes an angle ϕ with the XY plane, and if

θ' is the angle it makes with W and ψ' is the angle W makes with the X-axis, then after the collision,

$$\text{(for hit molecule)} \quad V' = W \sin \theta' ;$$
$$V'_x = V'(\cos \theta' \cos \psi' - \sin \theta' \sin \psi' \cos \phi) .$$
$$\text{(for hitting molecule)} \quad V = W \cos \theta' ;$$
$$V_x = V (\sin \theta' \cos \psi' + \cos \theta' \sin \psi' \cos \phi) .$$

Now consider Fig. 11. The center of the hitting molecule at the instant of collision must lie on the dotted spherical surface. Suppose its velocity is u. It can readily be shown that if the

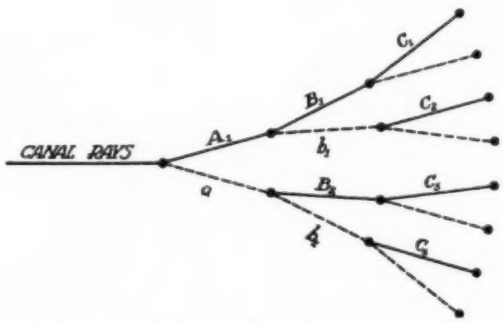

FIG. 13.—Various groups of secondary rays

spherical surface is divided into zones by coaxial cones whose angular apertures are given by the equations $\cos^2 \theta = \frac{a}{10}$, where a is an integral parameter varying from 1 to 10, the probability that the center of the hitting molecule will lie within any one zone is the same since the areas of projection of the zones on a plane perpendicular to u are all equal. It is further evident that if each zone is divided into two equally probable zones, the values of θ corresponding to the dividing cones will be equally probable values of θ. If then we assume ten collisions for a given value of u such that the line of centers at collision makes angles with u given by the equation $\cos^2 \theta = \frac{1}{20} + \frac{a}{10}$, where a has values 0, 1, 2, ... 8, 9, the resulting velocities after collision may be taken to represent fairly the actual distribution of velocities as far as θ is concerned.

These equally probable rays with the velocities of the hit and hitting molecules after collision in each case are shown in Fig. 11.

The general method of treating the problem in hand is shown in Fig. 12. Canal rays are assumed to enter the gas all moving with the same velocity u in the direction of the X-axis, which is also the line of sight. Let each ray collide with a gas molecule. The distribution of velocities among this first generation of secondary rays (A) can be fairly represented for our purpose by the ten

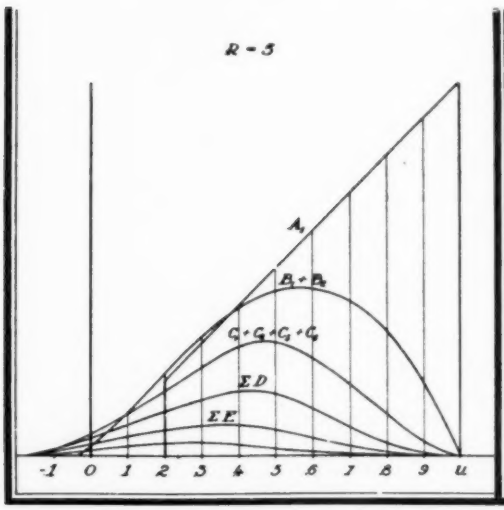

FIG. 14.—Doppler effect due to the various generations of secondary rays

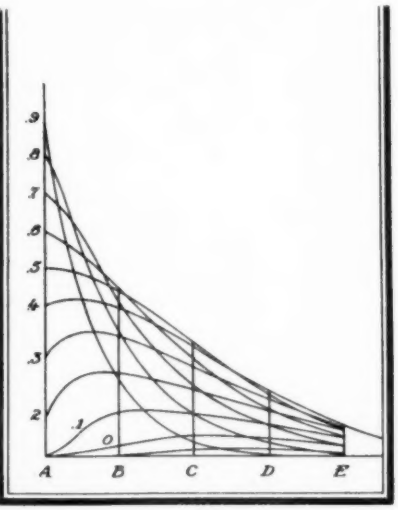

FIG. 15.—Extrapolation curves

bundles of rays shown in the XY plane, since the Doppler effect depends in this case only on the latitude angle θ and not on the azimuth angle ϕ. The energy given the hit molecules is shown in the lower half of the same diagram. It is seen that by far the most light is emitted by the molecules having a considerable velocity in the line of sight. Let us assume that the minimum energy necessary to produce ionization is some fraction, say one-fifth of the original energy of each canal ray. In that case two of our bundles of hit molecules will emit no light.

Now let each of these secondary rays strike gas molecules and produce another generation (B). Here the Doppler effect will

depend on both ϕ and θ; hence to represent this generation 1,000 bundles of hit molecules were chosen, ten values of ϕ (9° to 171° at intervals of 18°) for each value of θ and ten values of θ for each value of ψ, the angle which the hitting molecules made with the X-axis before collision. One hundred only are shown in perspective in the drawing. The energy and the x component of the velocity of each of these bundles was computed and the energy (taken equal to the square of the velocity) of all those whose

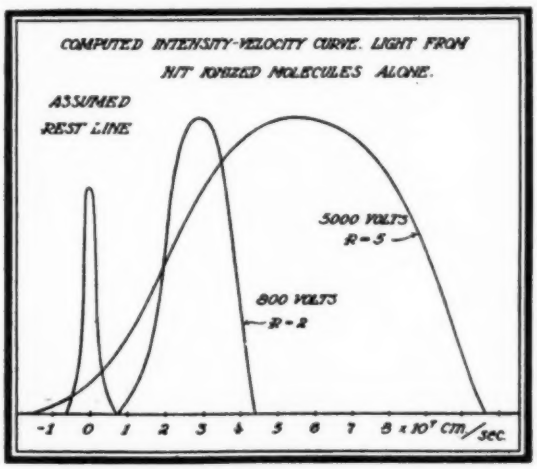

FIG. 16

velocity along the X-axis lay between certain limits, say 0.50 to 0.55 u, was added up and this sum was taken as proportional to the intensity of light corresponding to that Doppler shift. Thus the intensity of light as a function of the Doppler shift to be expected from this generation was obtained (B_1). In getting the effect of the third generation of hit molecules a multiplication by 100 was avoided by further extending the principle of representation. The cosine of ψ', the angle which each of the 1,000 rays of the second generation made with the X-axis, was computed by dividing the velocity of each ray along the X-axis by its actual velocity. Then these values of the cosine were plotted as a function of the velocity, one point for each ray. These were divided into groups of 20 and one ray picked out to represent each 20.

The values of the velocity and of the angles ψ' for each of these 50 representatives were obtained directly from the diagram. As in the case of B, ten values of ϕ for each of ten values of θ for each ray had to be considered, making 5,000 in all, for each of which the energy and velocity along the X-axis were computed. The intensity of light as a function of the Doppler shift was then obtained as before (C_1).

We have considered so far only the secondary rays in one direct

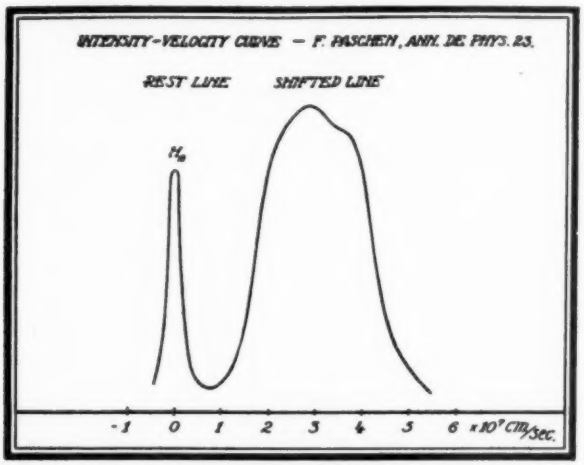

FIG. 17.—Stark effect for hydrogen

line of descent, both in the emission of light and in the production of other secondary rays. But surviving each generation of collisions there are in addition to the ionized hit rays some positively charged hitting rays which failed to ionize the molecules they hit but were merely deflected. In the diagram (Fig. 13), the full lines represent hit ionized molecules which emit light; the dashed lines represent ionized hitting molecules, emitting no light but capable of producing other ionized secondary rays which may emit light. In Fig. 21 the Doppler effect due to each of these groups of rays, on the basis of the assumptions made at the beginning, is shown (A_1, B_1, B_2, C_1, C_2, C_5, C_6) for the special case when the energy of each canal ray is taken to be five times the minimum energy necessary to produce ionization ($R=5$). The total Doppler effect

for each generation for the same case is shown in Fig. 14. The distribution of the light due to the later generations was obtained by extrapolation, using the curves shown in Fig. 15. The sum of them all shows discontinuities at $0.2\,u$ and u due to our artificial assumption that all the canal rays have the same velocity and that the unshifted line has no width. By modifying these assumptions to agree more closely with the actual facts the discontinuities are eliminated. The results for the cases when $R=2$

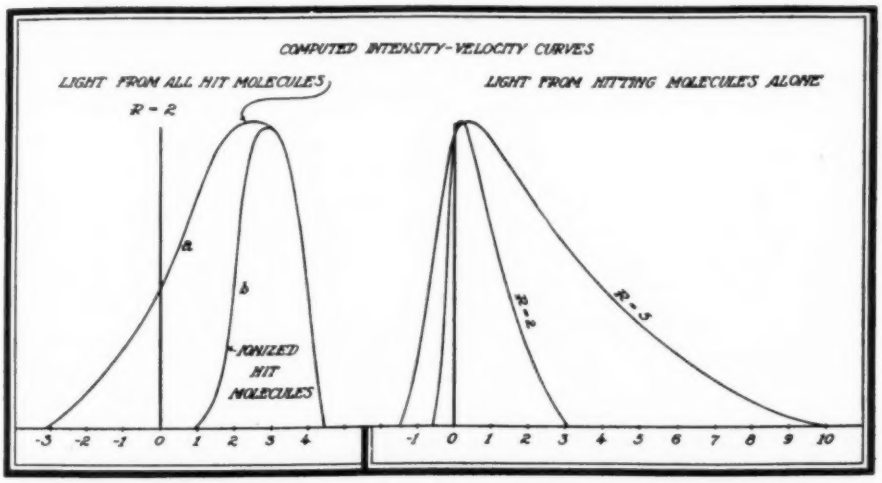
COMPUTED INTENSITY-VELOCITY CURVES
LIGHT FROM ALL HIT MOLECULES
R = 2
a
b
IONIZED HIT MOLECULES
-3 -2 -1 0 1 2 3 4
LIGHT FROM HITTING MOLECULES ALONE
R = 2
R = 5
-1 0 1 2 3 4 5 6 7 8 9 10

FIG. 18.—Testing the fourth assumption

FIG. 19.—Testing the fifth assumption

and $R=5$ are given in Fig. 16. It was assumed in getting the scale of abscissas that the minimum energy necessary to produce ionization corresponds to a velocity of 2×10^7 cm per sec. in the case of hydrogen. For comparison with the Stark effect as actually observed, an intensity-velocity curve has been reprinted from Professor F. Paschen's paper[1] (Fig. 17). This curve is obviously the sum of two, each having a marked intensity minimum and agreeing in general form with the two computed curves shown in Fig. 16. A better agreement could hardly be expected.

To show the necessity of introducing the fourth assumption regarding the minimum energy necessary to produce ionization,

[1] *Annalen der Physik*, **23**, 250, 1907.

a calculation was made without this assumption. The result for the case when $R=2$ is shown in Fig. 18 (*a*). Curve (*b*) is reproduced from Fig. 16 for comparison. Evidently the presence of the intensity minimum demands this fourth assumption.

The fifth assumption, that no hitting rays emit light as a result of the collisions, seems rather arbitrary and it appeared desirable to determine to what extent this assumption is necessary. If we assume that each of the charged hitting molecules emits light whose intensity is proportional to the energy of the collision, a calculation of the Doppler effect due to this light gives curves shown in Fig. 19. Here again the presence of the intensity-minimum in the Stark effect proves that, for the most part at least, the charged hitting rays do not emit any light corresponding to the series-line spectrum as a result of the collisions.

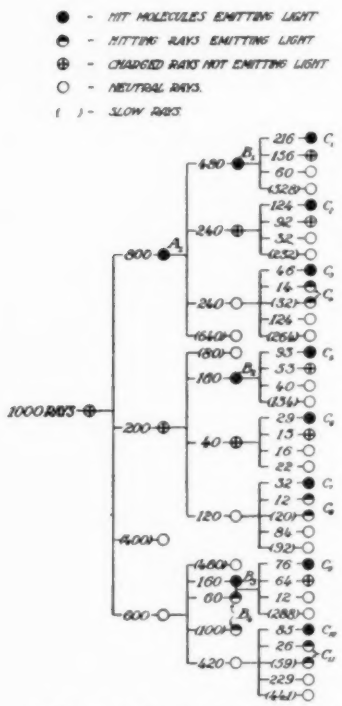

FIG. 20.—Genealogy on the basis of the six assumptions

There is still a further possibility to be investigated. Neutral rays when moving with sufficient velocity doubtless have the power of producing ionization. If so, the hitting molecule is as likely to be ionized as the hit molecule. Nothing is known as to the minimum energy necessary to produce ionization in this case, but a calculation was made adding the following assumption to the five considered above:

6. That when the collision of a neutral ray with a neutral gas molecule involves the transference of more than a certain minimum energy, which is assumed to be equal to the minimum energy necessary for ionization in the case of the collision of a charged

ray with a gas molecule, one will be ionized so that on the average half the hitting and half the hit molecules will emit light.

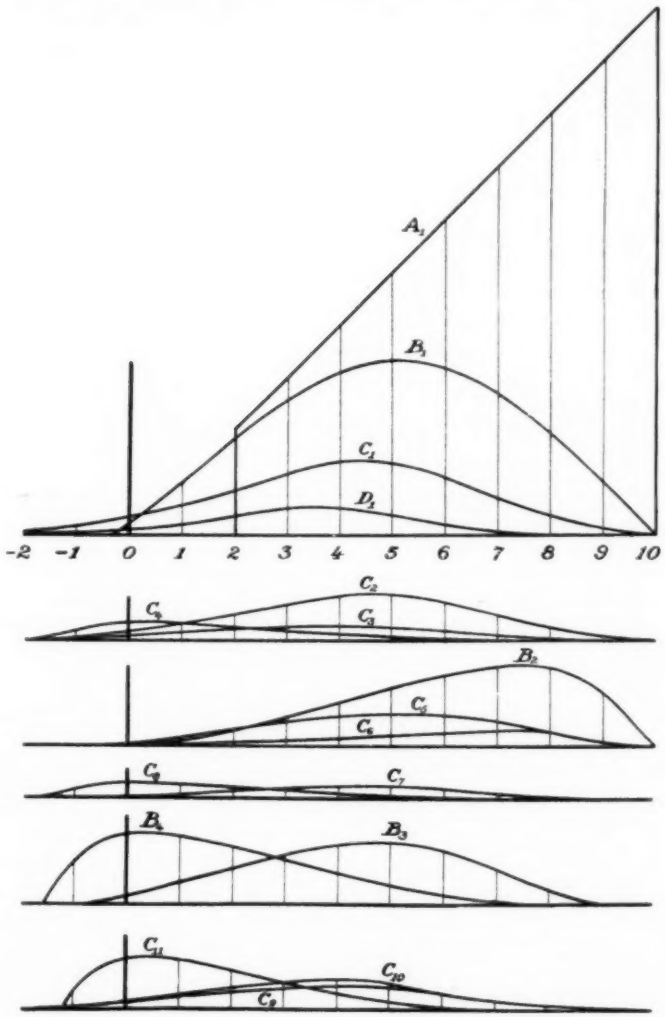

FIG. 21.—Doppler effect due to various groups of secondary rays

The introduction of this assumption adds several groups of active rays to our family. The genealogy for the case of 1,000 positively charged rays ($R = 5$) is given in Fig. 20. The slow rays

are those incapable of producing ionization. The Doppler effect corresponding to each of the active groups of rays was computed for the first three generations. The curves are shown in Fig. 21. The effect of later generations was obtained by extrapolation as above (cf. Fig. 15). The total effect due to 1,000 charged canal rays on the basis of the six assumptions is shown in Fig. 22 (*a*) for the special case when $R=5$. Curve (*b*) is reproduced from Fig. 16 for comparison. Evidently our sixth assumption is incom-

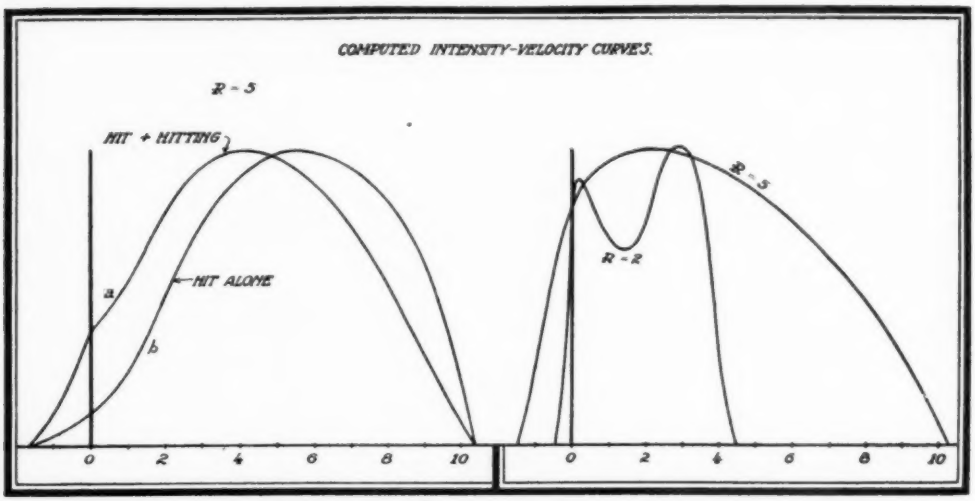

FIG. 22.—Testing the sixth assumption: charged canal rays

FIG. 23.—Doppler effect due to neutral canal rays on the basis of the sixth assumption

patible with an intensity minimum. Probably the minimum energy required for neutral rays to produce ionization is much greater than was assumed and hence the disturbing effect of these neutral rays is much less. Finally a computation was made of the Doppler effect to be expected on the basis of the six assumptions if 1,000 neutral canal rays with the same velocity u enter a gas. The genealogy in this case is similar to that shown in Fig. 20 except that the number in the various groups is quite different. The total Doppler effect due to these 1,000 neutral rays and their offspring is shown in Fig. 23 for the special cases when $R=2$ and

$R = 5$. Here again it is evident that the number of neutral rays must be small or assumption 6 must be incorrect. The number of neutral rays, however, is known from deflection experiments not to be small, may be in fact over 30 per cent of the whole number. The other alternative is therefore inevitable.

CONCLUSIONS

As a result of these calculations one seems justified in concluding that the original assumptions regarding the minimum energy necessary to produce the emission of light and the emission of light by the hit ionized molecules alone, represent the true state of the case very approximately. If the neutral rays produce ionization, the minimum energy required must be considerably greater than is necessary in the case of charged rays, so that the light thus produced is insignificant. The hitting charged rays must be neutralized for the most part and particularly when the shock of collision is great, at the same instant as the hit molecule is ionized.

If this analysis of the phenomenon is correct, a far more important conclusion necessarily follows: namely, that the series-line spectrum of hydrogen, which alone shows the Stark effect, is emitted by the positively charged molecule or atom of hydrogen. The reasoning leading to this deduction may seem rather indirect, but the fact that the above assumptions are the only ones, as I believe, which will explain the presence of the intensity minimum in the Stark effect on the basis of the experimentally verified hypothesis that the emission of light in the path of the canal rays is due to the collision of the canal rays and their offspring with gas molecules, is strong evidence for their correctness. Some other experiments will be performed shortly to test some further deductions from this analysis of the phenomenon.

DISCUSSION OF STRASSER'S RESULTS

Last April in the *Annalen der Physik*,[1] B. Strasser published some results of his painstaking experiments on the Stark effect which are of great interest. May I make a few suggestions as to a possible interpretation of these results along the same line as

[1] *Annalen der Physik*, **31**, 890–918, 1910.

in the case of the simple Stark effect in pure hydrogen? Here again one is working rather in the dark, but a suggestion may have value as a working hypothesis even though it later proves to be false.

Strasser has proved the following facts:

1. The intensity of the rest line in the Stark effect for hydrogen canal rays depends on the purity of the hydrogen in the tube. If the gas is sufficiently pure no rest line is obtained.

2. If a definite quantity of another gas is added, the intensity of the rest line is increased and that of the shifted line decreased in proportion to the amount added, so that when a sufficient quantity of the foreign gas is present an intensity minimum is no longer obtained between the two lines.

3. By experimenting with various gases (*N*, *Ar*, *He*) it was found that the effect produced when the partial pressure of the added gas had a certain value was greater the larger the atomic weight of the gas introduced.

From these results it seems evident that the rest line is due to the collision of hydrogen canal rays with molecules of the foreign gas. The larger the number of molecules of the foreign gas present, the greater the number of collisions with them in proportion to the number of collisions with hydrogen molecules which we have assumed produce the displaced line. If we assume that the charged canal rays are not neutralized for the most part by colliding with foreign gas molecules, but, because of the shock of the collision, emit light whose intensity is proportional to the energy of the collision, the Doppler effect due to this light would resemble in general form the curves shown in Fig. 19, though modified by the fact that in this case the hit molecules are larger. Thus we should expect the rest line to be broadened unsymmetrically toward the displaced line as Strasser found. Moreover gases with greater atomic weights may be expected to have larger molecules, hence to exert a greater influence for a given number of molecules by increasing the probability of being struck by the canal rays. The only difficulty is to explain why the hydrogen canal rays are neutralized for the most part when striking hydrogen molecules but not when hitting other molecules. But Strasser has shown that

the Doppler effect produced in the two cases is quite different. It is hard to see how the facts can be explained otherwise.

Strasser also reports that the light from the layer just in front of the cathode shows a weak displaced line with a distinct intensity minimum, whereas immediately behind the cathode a strong, broad displaced line is obtained corresponding to the high velocity of the canal rays. The explanation seems to be that as the canal rays acquire their velocity through the action of the electric field in front of the cathode, and since the potential-gradient is extremely steep right near the cathode, the number of rays having a velocity high enough to produce ionization must increase very rapidly just at the surface of the cathode. This leads to the apparent discontinuity at the surface of the cathode which suggested Strasser and Wien's theory that the canal rays emit light as a result of the electric shock experienced in suddenly passing from a very strong to a weak field—a hypothesis which of course is no longer tenable.

Strasser's observation that the hydrogen lines persist to a greater distance from the cathode than the lines of a foreign gas may be interpreted to show that the minimum energy required to produce the emission of light is greater in the case of the foreign gas than in the case of hydrogen. Finally Strasser reports that the spectrum of the light from the path of the canal rays viewed normal to their velocity shows the same broadening of the lines whether there is a foreign gas present or not. The computation of the effect to be expected on the basis of the five assumptions made above is extremely laborious, but a first approximation shows a fair qualitative agreement with this experimental result. Strasser does not publish quantitative data as to the amount of the broadening. His more recent results for hydrogen canal rays in nitrogen gas[1] can obviously be explained in the same way as these earlier ones and merely add to the evidence in support of this analysis of the phenomenon.

CONDUCTION OF HEAT BY A GAS AT LOW PRESSURE

The curves shown in Figs. 5 and 6 give some idea of the relative importance of convection and of radiation in the cooling of the

[1] *Annalen der Physik*, **32**, 1107, 1910.

cone. Curve XIV was made with the pressure in the canal chamber about twenty times as great as when curve XXI was taken. From the derivative curves (Fig. 6) it appears that the energy being received by the cone per second in the first case is even greater than in the second case yet the maximum temperature reached in the first case is only one-sixth of that reached in the second case. By differentiating equation (3) we get

$$c\left(\frac{d^2x}{dt^2}\right)_+ = -(A+B+C)\left(\frac{dx}{dt}\right)_+ \qquad (5)$$

since the time derivatives of z and y are negligible in comparison with $\frac{dx}{dt}$ for the cooling part of the curve. This enables a rough determination of $(A+B+C)$ to be made. The results show a surprisingly good agreement. The value increases from 0.00010 calories per second at a pressure of 0.005 mm to .00065 calories per second at a pressure of 0.1 mm of mercury—showing that at the higher pressure convection is at least ten and probably twenty times as important as radiation in cooling the cone. The conduction coefficients computed from these data are about 0.00017 and 0.000013 calories per second per degree C. per cm^3 for pressures of 0.1 mm and 0.005 mm respectively. By comparison with the coefficient for hydrogen at ordinary pressures, which is given by Meyer as 0.00040, it is seen that the coefficient must be nearly constant until pressures below 1 mm of mercury are reached.

PROPORTION OF NEUTRAL CANAL RAYS

From the energy-flux of the canal rays it is possible to compute the lower limit to the number striking the cone per second by dividing the energy-flux by the energy a singly charged molecule would have if acted on by the whole cathode-fall of potential. Curve I of Fig. 24 was thus computed from the curve in Fig. 8. A lower limit to the number charged could be obtained by dividing the current carried by the rays to the cone by the known value of e. Thus curve II was computed. The increase in the proportion of neutral rays with the increase in the cathode-fall of potential is to be expected, I think, for the particular form of tube which was used, though it might not be true for another tube.

DESCRIPTION OF APPARATUS (*Continued*)

To admit the gas at a uniform rate a capillary was used. The best dimensions were determined by the use of Knudsen's formula.[1]

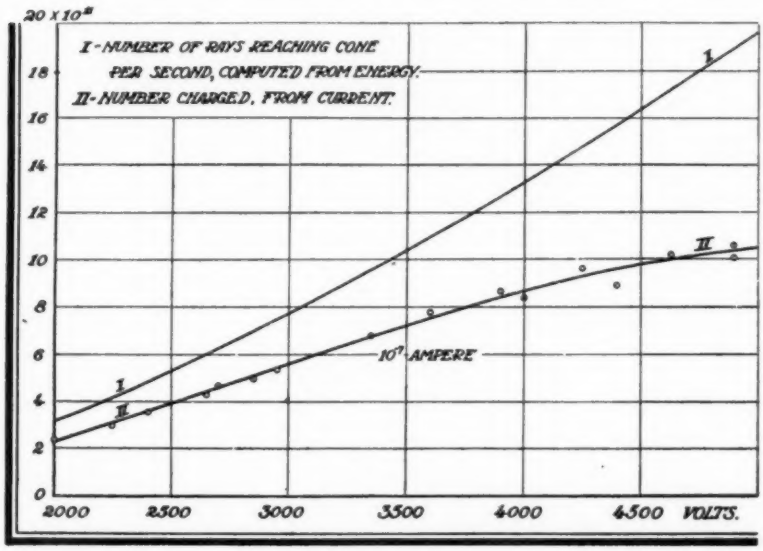

FIG. 24

A number of capillaries were drawn and left attached to tubes of larger bore. The method employed to measure them is illustrated in Fig. 25. Mercury was forced part way into the capillary from the larger tube by means of a rubber bulb, and the capillary was

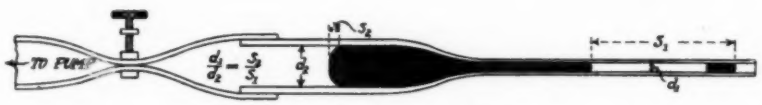

FIG. 25.—Calibration of a capillary tube

calibrated by noting the corresponding distances moved by the two ends of the mercury thread. From the diameter of the larger tube that of the smaller could thus be readily calculated.

To remove the mercury vapor from the gas, it was passed first

[1] *Annalen der Physik*, **28**, 75, 1909.

through flowers of sulphur and copper filings to remove the sulphur vapor, then through a coil kept immersed in a freezing mixture of solid CO_2 and ether. All traces of the mercury lines disappeared from the discharge tube with this arrangement.

To secure a continuous discharge the following arrangement was hit upon (see Fig. 4). An induction coil was used to keep a capacity charged by periodically sparking across a point-and-plane gap made sufficiently long to prevent back sparking. This capacity of 0.3 microfarad was allowed to discharge slowly and continuously through a resistance of several hundred thousand ohms (made by rubbing graphite on a plate of ground glass) which was placed in series with the discharge tube. No discontinuities in the discharge could be observed with a rotating mirror. After the induction coil was stopped the luminous discharge would continue sometimes for over a minute. The discharge therefore was probably quite continuous. If the induction coil was connected to the tube directly, producing a discontinuous discharge, a spark-gap of one centimeter would short-circuit the tube if placed in parallel with it, whereas if the condenser set-up was used with the same pressure in the tube, 5,000 volts would maintain a continuous discharge through the tube. Other evidence was secured tending to show that a continuous discharge is an unstable phenomenon unless very special precautions are taken; and that the mean discharge potential is always greater in the case of the discontinuous discharge than in the case of the continuous, though much less than the maximum value reached by the oscillating potential-difference in the former case. A rotating mirror should always be used to test the continuity of the discharge in the case of any quantitative experiments involving the discharge of electricity through gases.

My thanks are due to Professor A. G. Webster for his interest and encouragement during my stay at Clark University, where the above theory was developed.

MADISON, WIS.
December 28, 1910

ADDENDUM

By a strange mischance I failed to see, until the above was in type, an article by Professor W. Wien[1] in which he gives results of his measurements of the luminosity of a canal-ray beam at various pressures. He finds that the luminosity increases with the pressure, in qualitative agreement with my observations, but a quantitative comparison would be difficult because of the complexity of the conditions existing with his form of apparatus. His interpretation of the results is quite different from mine.

[1] *Annalen der Physik*, **30**, 349–368, 1909.

THE SPECTRA OF SPIRAL NEBULAE AND GLOBULAR STAR CLUSTERS[1]

SECOND PAPER

By E. A. FATH

The first paper on this subject by the writer describing results of work with the Crossley reflector was published in *Lick Observatory Bulletin*, **5**, 71, 1909. From the evidence presented it was concluded: first, that the spectra of spiral nebulae vary from one in which the bright lines of the gaseous nebulae are the most prominent feature to one closely analogous to that of the sun; second, that the *Hercules* cluster probably contains stars of various types, while the clusters N.G.C. 7078 and 7089 are composed of stars predominantly of the F type.

The present paper will take up the results I have obtained at this observatory with the 60-inch reflector.

The spectrograph used had been designed for other purposes and is not well adapted to this kind of work. Suffice it to say that there is great loss of light due to (1) reflections from two plane mirrors; (2) a large amount of unnecessary glass in the optical system, as the lenses are ordinary commercial portrait lenses; (3) the impossibility of keeping the slit at the focus of the telescope if the focus changed owing to changes of temperature after the exposure had started. Owing to these various factors the exposures were longer than otherwise necessary. It is hoped that all these difficulties will be overcome in a new spectrograph which is soon to be constructed especially for this class of work.

The spectra are but little over 3 mm long from λ 5000 to λ 3700. This small scale and the coarse grain of the Lumière "Sigma" plates, which were used on account of their speed, make the information derived very meager indeed.

The plates were measured by means of a Hartmann spectrocomparator. As standards, stellar spectrograms with well-defined

[1] *Contributions from the Mount Wilson Solar Observatory*, No. 49.

lines taken with the same dispersion were used. The more conspicuous lines found coincident in the standard plate and the one being measured were assumed to be identical, and the remaining lines interpolated between those thus assumed as known. The former are indicated by letters, the latter by their wave-lengths.

The two divisions of the subject will be treated separately.

SPIRAL NEBULAE

The following table gives the data in regard to the exposures on the three nebulae whose spectra will be described.

N.G.C.	Dates of Exposure	Total Exposure
650–51	1909 Sept. 22, 23	10^h47^m
4725	1910 Feb. 15, 16	9 58
4736	Feb. 10	7 00

N.G.C. 650–51.—As stated in my former paper the evidence that this nebula is a spiral is not conclusive, although the presumption in favor of this is fairly strong. At that time the only thing known about its spectrum was an observation by Huggins,[1] who had found the bright nebular line at 501 $\mu\mu$. The present plate shows seven bright lines at

373 $\mu\mu$	486 $\mu\mu$
387	496
397	501
434	

which correspond to lines commonly found in gaseous nebulae. The plate is somewhat underexposed, but so far as can be determined there is no background of continuous spectrum.

N.G.C. 4725.—This spectrum is of the solar type. Lines corresponding to F, G, H, and K are present.

N.G.C. 4736.—The spectrogram of this nebula is not at all like the one obtained at Mount Hamilton. The present plate shows a solar-type spectrum with the following seven lines: F, G, 425 $\mu\mu$, 410 $\mu\mu$, H, K, 387 $\mu\mu$. The Mount Hamilton plate shows a broadening and brightening at 406 $\mu\mu$, which was assumed to be a bright

[1] *Phil. Trans.*, **156**, 381, 1866.

band, and absorption lines at 387 and 400 $\mu\mu$, but the plate is not in good focus. A comparison of the two plates in the spectrocomparator shows certain points of resemblance, yet it is hard to see how a change in focus alone could be responsible for the enormous difference in appearance. The Mount Wilson plate is in good focus and the solar type of spectrum cannot be questioned. As it does not seem probable that the nebula itself changed in the brief interval, about twenty months, between the two exposures, another plate will be taken as soon as possible.

Plates have also been taken of the spectra of other spirals and also of some clusters which are underexposed to such an extent that no reliable conclusion can be drawn from them.

The star-cluster theory of the spirals is thus given additional support by the solar-type spectra of N.G.C. 4725 and 4736.

Although it may appear somewhat premature, it may not be altogether out of place to suggest that we have some evidence, slight though it is, of a progressive change in the spectra of the nebulae with change of form. The beginning of the series would be an irregular nebula such as the great one in *Orion* with a bright-line spectrum. The second type might be a probable spiral like N.G.C. 650–51. The spectrum consists of bright lines and little or no continuous background. The third type is a planetary, such as N.G.C. 6543, with the same type of bright-line spectrum and considerable continuous spectrum,[1] but of quite definite spiral or helical form. This form is partially brought out in Plate 57 of Vol. **8** of the *Lick Observatory Publications*, but is much more clearly shown in a recent negative, taken by Mr. Ritchey with the 60-inch reflector, which is soon to be published and to which I am permitted to refer. The fourth type is a well-developed spiral like N.G.C. 1068,[2] which, while still showing bright lines in its spectrum, has also a strong continuous spectrum which contains absorption lines. The last type, in which the nebula has condensed to stars, at least near its center, is illustrated by nebulae giving solar-type spectra such as the *Andromeda* nebula,[2] N.G.C. 4725

[1] *Astronomy and Astrophysics*, **12**, 51, 1893, and *Lick Observatory Bulletin*, **2**, 51, 1902.

[2] See my first paper.

and 4736. There are great gaps in such a series as outlined. Whether or not these types represent steps in the development of nebulae is certainly a debatable question. If such a series does represent successive stages in the life-history of the nebulae, then star clusters should be added as the final product. This suggestion is advanced with the hope that it may prove of service until something better can be obtained.

GLOBULAR STAR CLUSTERS

Spectrograms of eight globular star clusters have been obtained as follows:

N.G.C.	Dates of Exposure	Total Exposure
5024	1910 May 1	6^h45^m
5272	Apr. 30	5 00
6205	June 15	6 25
6229	June 16, 17	11 45
6341	June 14	3 27
6656	Aug. 11, 12	9 15
6934	Aug. 29, 30	8 00
7078	1909 Aug. 22, 23, 24	16 15

The plates of N.G.C. 6656 and 6934 are considerably underexposed.

The lines found are given in the following table. A line whose presence is very probable but not certain is followed by a question

N.G.C. 5024	5272	6205	6229	6341	6656	6934	7078	7089
Hβ	*Hβ* (?)	*Hβ* (?)	*Hβ* (?)	*Hβ*	*Hβ*	....	*Hβ*	*Hβ*
							444	
Hγ	*Hγ*	*Hγ*	*Hγ*	*Hγ*		*Hγ*	*Hγ*	*Hγ*
418	421	420	420 (?)	419		421	417	
Hδ	*Hδ*	*Hδ*	*Hδ*	*H δ*	*Hδ*	*Hδ* (?)	*Hδ*	*Hδ*
				402				
Hε or H	H	*Hε* or H	H	H	*Hε* or H		H	H
	K		K	K	K (?)		K	K
			Hζ	*Hζ*			*Hζ*	*Hζ*
							Hη	*Hη*
							Hθ	*Hθ*

mark. This uncertainty is due to one of two causes: either the plate is not very strong, and therefore lines at either end of the

the spectrum are weak, or there is a local defect in the plate. N.G.C. 7089, described in the first paper, has been added for purposes of comparison.

A most interesting result is here shown. The globular clusters investigated have almost identical spectra, approximately F type. The hydrogen series predominates, but in every case where the spectrum extends far enough into the violet strong H and K lines of calcium are found. The line near 419 $\mu\mu$, present in nearly all, should more properly be called a band. It is faint and diffuse and undoubtedly corresponds to a large group of lines found in this region in F-type stars. It varies somewhat in position depending on the relative strength of the components. This identification of type must be taken with due precaution. It means simply that the clusters give a spectrum lying between A and G in the Harvard series B, A, F, G, K, M. Furthermore, I am quite certain that only the brighter stars in the clusters are concerned in the spectra obtained. In some instances, when setting on a comparatively coarse cluster, the light from a small group of the brighter stars could be seen coming through the slit. The light from the background of faint stars was almost negligible in comparison.

There is another consideration, however, which must not be overlooked. In the first paper it was stated that from the Mount Hamilton plate the *Hercules* cluster, N.G.C. 6205, gave evidence of containing stars of different spectral types. The Mount Wilson plate does not support this, as it shows only the F type. Both plates must be considered. As the slit in the two cases almost certainly crossed different portions of this cluster the necessity of obtaining more than one plate of each cluster is shown.

I have been able to form a list of 109 so-called globular clusters, some of them very coarse. The nine more condensed ones listed in the table are scattered over a region from 13^h to $21^h.5$ of right ascension and from $+48°$ to $-24°$ in declination. We thus have what might be called a fair sample.

It would be of great interest to know whether there is a connection between the type of spectrum and the number of cluster variables or not. Out of the nine clusters in the above list at least four contain variable stars, namely N.G.C. 5272 with 132 variables,

6656 with 16, 7078 with 51, and 7089 with 10.[1] No direct connection appears; nevertheless it is quite possible that when spectrograms of higher dispersion are available we may be able to establish such a relationship.

In conclusion I wish to express my thanks to Director Campbell of the Lick Observatory for sending me the Mount Hamilton negatives to allow a direct comparison of the two series of plates.

MOUNT WILSON SOLAR OBSERVATORY
December 23, 1910

[1] *Annals Harvard College Observatory*, **60**, 217, 218.

SOME RESULTS OF A STUDY OF THE SPECTRA OF *SIRIUS*, *PROCYON*, AND *ARCTURUS* WITH HIGH DISPERSION[1]

By WALTER S. ADAMS

Among the accessory instruments to be used with the 60-inch reflecting telescope at Mount Wilson as originally planned by Mr. Hale was included a large spectrograph to be employed in photographing the spectra of some of the brighter stars. The instrument was completed shortly after the erection of the large reflector and has been in use by us for nearly a year. The telescope in this case is employed in the coudé form, and the light is reflected downward through the hollow polar axis to the slit of the spectrograph, the equivalent focal length being 150 feet (45.7 m). The ratio of aperture to focal length accordingly is 1 to 30. It was at first planned to mount the spectrograph on an inclined pier with its axis in the line of the center of the polar axis, but for the sake of convenience in manipulation and constancy of temperature it was decided later to place the instrument in a vertical position in an underground pit. This of course necessitates an additional reflection, which is obtained by means of a small plane mirror supported from the lower bearing of the axis of the telescope.

The spectrograph as used by us at present is of the auto-collimating type. It consists of a lens of 15.2 cm aperture and 5.5 m focal length, used in conjunction with a dense flint-glass prism of 63° angle and a plane mirror to return the light through the prism. Unfortunately, we have as yet been unable to secure a satisfactory prism of sufficient size to utilize the full beam from the collimating lens. The one employed is 20.3 cm long, but has a face only 12.7 cm across. Accordingly, in the position of minimum deviation, it is completely illuminated by a beam only 5.7 cm in cross-section, and there is a loss of light of over 50 per cent. With a prism of full size it will be possible to reduce the exposure times greatly, and thus bring additional stars within the range of the spectro-

[1] *Contributions from the Mount Wilson Solar Observatory*, No. 50.

graph. The approximate linear scale of the photographs obtained is as follows:

1 mm = 1.4 Å at λ 4300,
1 mm = 2.4 Å at λ 5000,
1 mm = 6.2 Å at λ 6500.

The plates used are 43 cm in length and the definition is satisfactory over their entire extent. In the more refrangible part of the spectrum the Lumière "Sigma" plates have been used for the most part, while to the red of λ 4900 Seed "Gilt Edge 27" plates, sensitized by Wallace's formula, have proved very satisfactory. The spectrum of the iron arc has been used for comparison purposes.

We have up to the present time secured with this instrument spectra of the following stars: *Sirius*, *Procyon*, *Arcturus*, *α Orionis*, *β Orionis*, and *α Scorpii*. In this note I wish to refer briefly to some results obtained from a study of the photographs of the first three of these stars. The plates have all been taken by Mr. Babcock and myself.

The material available for investigation includes six plates of *Sirius*, covering the spectrum from λ 4200 to λ 6600; four plates of *Procyon* from λ 4200 to λ 4900; and nine plates of *Arcturus* from λ 4300 to λ 6600. A few of the earlier plates of *Arcturus* were taken with a very wide slit, and are entitled to considerably less weight than those obtained more recently.

The chief object in the study of these spectra has been to test the possibility of detecting difference of displacement for the different lines, and thus obtain some knowledge of the effective pressure in the atmospheres of these stars. Accordingly, the lines used for measurement have been selected with this object in view. In the case of *Sirius* the total number of suitable lines is comparatively small, and so practically all of the lines have been measured for which satisfactory comparison lines for use in the derivation of wave-lengths are available. In the case of *Procyon* and *Arcturus* the selection has been made largely on the basis of the list of lines used by myself in an investigation of the displacements of lines at the sun's limb. Since it seems highly probable that these displacements are due mainly to pressure, this list is very well adapted for the object in view. The fact that the enhanced lines

as a class show larger shifts at the sun's limb than do the ordinary arc lines makes these of particular importance in the study of the stellar spectra, and so practically all of them have been measured upon our plates. As is well known, the enhanced lines are extremely prominent in the spectrum of *Sirius*, a comparatively small number of arc lines being represented, and these as a rule only from among the strongest. In the spectrum of *Procyon* the enhanced lines, though still very strong, are relatively less prominent, and the arc lines are much more numerous. The spectrum of *Arcturus* strongly resembles that of sun-spots, and the enhanced lines are weaker than they are in the sun. The immense number of suitable lines available for measurement in the spectrum of *Arcturus* has made its study of especial interest.

In a summary given below I have collected the results of the measures of the arc and the enhanced lines for the three stars.

TABLE I

Plate	No. Meas.	Region	Enhanced Lines		Arc Lines	
			Δ	Number	Δ	Number
Sirius						
α 20	2	λ 4450	+0.26	37	−0.38	36
25	2	4550	+0.38	36	−0.28	49
27	2	5600	+0.75	6	−0.21	25
34	1	4550	+1.28	37	−0.60	51
41	1	4600	+0.41	33	−0.43	33
Procyon						
α 29	2	λ 4550	+0.37	33	−0.16	108
35	1	4600	+0.11	23	−0.32	60
36	1	4550	+0.49	37	−0.24	132
42	3	4700	+0.42	20	−0.17	49
Arcturus						
α 46	1	λ 4600	+0.13	34	−0.03	156
Mean of 7 plates			+0.03	52	0.00	559

The quantities given under Δ are in kilometers, and represent the differences between the mean of all the lines measured and the enhanced and the arc lines respectively. The positive sign denotes displacement toward the red: thus, in the first case, the value +0.26 indicates that the average velocity given by all the enhanced lines differs from the mean of all the lines measured by 0.26 km. If the measured velocity is negative the enhanced lines show a

smaller numerical value, if positive a larger. In other words, the enhanced lines are displaced to the red relative to the arc lines. The plates have been measured by Miss Lasby, Miss Ware, or myself, and the table indicates whether any given plate has been measured by one or more of us. One plate in the less refrangible part of the spectrum of *Sirius* has been omitted, as the enhanced lines are too few in number to give a determination of any material value.

In the case of *Arcturus* the results from a considerable number of plates are combined in order to obtain a sufficient number of enhanced lines to give a determination of reasonably high weight.

The persistence of the positive sign in the values for the enhanced lines for *Sirius* and *Procyon* is certainly remarkable, and in view of the difficulty of the measurements the absolute values of enhanced−arc lines, with the exception of one plate of *Sirius*, are tolerably accordant. The very large value given by a 34 is probably due mainly to the inferior quality of this plate and the fact that only one measurement is available for it. If we combine the results, assigning weights according to the number of measures and the number of lines, we obtain the following final values:

Sirius: Enhanced−Arc Lines $\Delta = +0.90$ km $= +0.014$ Ångström,
Procyon: Enhanced−Arc Lines $\Delta = +0.58$ km $= +0.009$ Ångström,
Arcturus: Enhanced−Arc Lines $\Delta = +0.08$ km $= +0.001$ Ångström.

The value for *Arcturus* is of comparatively low weight, and mainly interesting as being practically negligible in size.

Before discussing these values further I wish to add the results obtained from an investigation of the lines of different elements in the spectrum of *Arcturus*. As already stated, the spectrum of this star resembles strongly that of a sun-spot, a large portion of the lines of titanium, vanadium, and calcium being greatly strengthened, the enhanced lines weakened, and those of iron and chromium either strengthened or weakened according to their behavior under different conditions of temperature. The only apparent exception seems to be nickel, the lines of which are rather more prominent in the spectrum of the star than in sun-spots.

The following short table shows the values derived for the lines of the different elements from a combination of all the plates.

The weights are obtained by adding together the products of the number of lines measured by the weights of the separate plates. As in the case of the enhanced lines, the quantities represent the differences between the mean values given by all the lines and the mean values for the lines of each element. The positive sign denotes displacement toward the red.

TABLE II

Element	km	Angström	Weight	No. Lines
H	−1.2	−0.020	22	8
Ca	−0.70	−0.017	209	78
Mg	−0.68	−0.011	54	22
V	−0.24	−0.006	162	58
Ti	−0.23	−0.006	451	165
Ni	−0.22	−0.006	107	42
Fe	+0.25	+0.006	1222	440

According to the results of this table the lines of iron are shifted toward the red with reference to those of all other elements investigated. Among the latter titanium, vanadium, and nickel fall in one group as regards differences from the mean, calcium and magnesium in another, while hydrogen gives the largest value of all.

A definite conclusion as to the reality of these differences, as well as those found for the enhanced lines, must of course be determined by the degree of precision obtained in the measurement of the plates. A calculation of the probable error of a single line from average plates of *Sirius*, *Procyon*, and *Arcturus* gives the following results:

Sirius, 37 enhanced lines $r = \pm 1.00$ km,
Procyon, 26 enhanced lines $r = \pm 0.69$ km,
Arcturus 51 iron lines $r = \pm 0.51$ km.

The probable error accordingly for the mean of a group of 30 lines for *Sirius* will be ± 0.18 km, for *Procyon* ± 0.13 km. If we enter the tables of the probability integral with these values we find approximately: for *Sirius*, the probability that on a single plate two groups of lines, each 30 in number, will differ from each other by 0.64 km owing to errors of measurement is about 1 to 25; for *Procyon*, that two similar groups will differ by 0.43 km the probability is 1 to 17. These values are for single plates, and for the series of plates the probabilities are greatly decreased.

In fact, for the mean values of +0.90 km for *Sirius*, and +0.58 km for *Procyon*, the probability is less than 1 to 1,000 in each case.

A similar calculation in the case of *Arcturus* is of interest as indicating which of the differences from the mean found for the different lines are to be considered as least liable to be due to accidental errors of measurement. If we assume the same degree of accuracy of measurement for the other elements as for iron we find that the probability of the reality of the differences is strongest in the cases of hydrogen, calcium, magnesium, titanium, and iron. While these considerations are largely theoretical, they serve the purpose of indicating the great strength of the presumption that the majority of the differences found for the enhanced lines in *Sirius* and *Procyon*, and among the lines of different elements in *Arcturus*, are to be considered as genuine and not due to accidental causes.

Attention should be called to one point in connection with these results, which I at first thought might have affected the values materially, especially in the cases of *Sirius* and *Procyon*. Since the iron arc was used for comparison purposes, a large number of the stellar arc lines measured in these two stars are direct comparisons with the stronger lines in the comparison spectrum. The enhanced lines, on the other hand, are of course indirect comparisons. If any systematic effects were present in measurements on the stronger comparison lines, they might affect the average of the arc lines more than the enhanced lines. To guard against this possibility the photographs have been reduced by the aid of a residual curve with which the wave-lengths have been corrected. A comparison of the results with those obtained by linear interpolation showed no differences for the mean of arc or enhanced lines on any plate amounting to as much as 0.1 km.

We may now pass to the discussion of these results. Perhaps their most striking feature is the remarkable resemblance they bear to the displacements found in the investigation of the lines at the sun's limb. In the latter case it was found that the enhanced lines were shifted to the red relative to the arc lines. Also that the shifts of the titanium, vanadium, and calcium lines were much less than those of the iron lines, while hydrogen showed no appreciable displacement. A similar shift of the enhanced lines

is found here in the cases of *Sirius* amd *Procyon*, and possibly to a very slight degree in *Arcturus*. In the last-named star, moreover, we find a precisely similar behavior on the part of titanium, calcium, hydrogen, and the other elements referred to. The only exception to the analogy is nickel, and attention has already been called to the somewhat greater prominence of this element in the spectrum of *Arcturus*. It seems altogether probable, therefore, that the cause giving rise to the systematic differences in the stellar spectra is the same as that which produces the displacements observed at the sun's limb. Since the latter have been ascribed mainly to the effects of pressure by those who have investigated the subject, we may consider pressure as the principal agent in causing the displacements observed in the stellar spectra.

According to the laboratory investigations of Humphreys and others, the arc lines of iron are shifted under pressure an average amount of 0.0025 Å per atmosphere. Now at the sun's limb the enhanced lines in the more refrangible part of the spectrum are shifted approximately 50 per cent more than the arc lines. Recent investigations by Gale of the spectrum of the titanium spark under pressure made at this observatory indicate that the enhanced lines of this element are shifted on the average about 50 per cent more than the arc lines at the same pressure. Accordingly, if we may conclude, as seems very probable, that the same relationship holds between the arc and the enhanced lines for other elements as well, we are provided with a means of determining the pressure in the atmospheres of stars whose spectra show such displacements. The enhanced lines in the spectrum of *Sirius* are shifted toward the red relative to the arc lines by 0.014 Ångström. This would correspond to a pressure of 12 atmospheres, or, since solar wave-lengths are used for the comparison lines, the pressure in the atmosphere of *Sirius* would be 12 atmospheres greater than that in the sun's reversing layer. Similarly, in the case of *Procyon* we obtain a pressure 7 atmospheres greater than that in the reversing layer. These results are especially interesting when discussed in connection with the spectra of the two stars. The hypothesis has sometimes been suggested, based on considerations of average density and intrinsic luminosity, that no true photosphere of the solar type

exists in the case of *Sirius*, but rather that the star consists of a vast mass of gas increasing in density toward the center but with no actual surface of condensation. In such a star it is evident that the light could come from regions extending to great depth in which the pressures are high, and this appears to agree with the results found. Similarly, in the case of *Procyon* the character of its spectrum indicates a condition between *Sirius* and the sun, and we find the amount of pressure intermediate between the two.

The results for *Arcturus* are of quite a different sort but of considerable interest. Although its spectrum is almost identical with that of a sun-spot, it is hardly probable that this is due to its being covered with sun-spots, but rather to the fact that the general conditions in its atmosphere are similar to those in sun-spots. Accordingly, we should expect that this star would have a definite photosphere, and that the light emitted by it would come from comparatively shallow depths, and so from regions of relatively low pressure. Furthermore, the results found for the lines of different elements in *Arcturus* indicate a general arrangement of the gases in the star's atmosphere very similar to that in the sun. Thus we know that in the sun hydrogen gas rises to very great heights, and the gases of magnesium and calcium (leaving out of consideration in the latter element the H and K lines) also attain a high level, though somewhat lower than hydrogen. Similarly, titanium is a relatively high-level element, but iron is low. In *Arcturus* we find the lines of all of these elements giving wave-lengths shorter than those of iron, the greatest difference being for hydrogen, the next for magnesium and calcium, and the least for titanium. It seems reasonable, therefore, to conclude that the lines of these elements are subject to less pressure than are those of iron, and consequently that the gases producing them lie at a higher average level.

The latter part of this discussion is naturally somewhat speculative in character, particularly as regards the magnitude of the pressures in the stellar atmospheres. This investigation serves, however, to indicate what may prove to be a satisfactory method of attack on this interesting problem.

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THE CATHODE-RAY FLUORESCENCE OF SODIUM VAPOR

By R. W. WOOD and R. H. GALT

The vapor of metallic sodium, because of its remarkable optical and spectroscopic properties, has been the subject of an extended investigation by one of the present writers, which has furnished much information regarding the mechanics of molecular radiation. When subjecting the vapor to an electric discharge, a new spectrum was accidentally discovered entirely different from anything ever seen in the flame, arc, or spark, and appearing to have no relation to the absorption, fluorescent, or magnetic rotation spectra. Cathode rays were found to excite, in addition to the green fluorescence stimulated by white light, a curious banded spectrum in the red, orange, and yellow region. A report of preliminary work on this spectrum was published in the *Philosophical Magazine* ([6] **15**, 581, 1908) and in the *Proceedings of the American Academy of Arts and Sciences* (November 1908). The present paper is a further study of this spectrum.

APPARATUS

The apparatus was essentially that described in the papers referred to. The vacuum tube was made of steel, 36 cm long and 3.3 cm in diameter, and was connected by a side tube to a mercury pump. As shown in the diagram, the steel tube itself formed the negative electrode; the positive electrode was a nickel wire carried in a glass cap cemented with sealing wax to one end of the steel tube. The electric discharge passed from the end of the nickel rod, inside the steel tube. The opposite end of the steel tube was closed by a glass or quartz window.

The current was obtained from an induction coil, built by the Roentgen Co. of Philadelphia, capable of sparking ten inches in air. The potential could be adjusted over a very wide range a (from $\frac{1}{4}$-inch spark to a 10-inch one) by almost infinitesimal steps. For

any given spark-gap, the tension on the interrupter could be so adjusted that the sparks were intermittent, perhaps one for every four or five interruptions of the current, dependent on very slight accidental variations in the resistance of the contact points.

Two spectroscopes were used. To obtain the general characteristics of the spectrum, a quartz spectrograph, made by Fuess, was employed; for accurate wave-length determinations, a Rowland concave grating of six feet radius of curvature, 15,000 lines to the inch, was used. The plates used for the red and yellow were Cramer's new "Spectrum" plates; for the green and blue, Wrattan and Wainwright's "Green Sensitive" plates.

The steel tube was heated by means of a single bunsen burner, the use of a hotter flame being prohibited by two facts: the higher the temperature, the longer was the column of vapor, and hence the greater the absorption; and, with too intense heating, it was impossible to keep the ends of the tube cool enough to prevent a fog from forming on the window. Cooling was effected by pouring cold water on cotton batting wrapped around the tube near the ends.

To obtain the spectrum, a piece of clean sodium (about 8 cubic cm) was placed in the middle of the tube, and the pressure reduced to a few millimeters of mercury. The flame was then applied and the pumping continued until most of the occluded hydrogen was removed. When the pressure was 2 mm or higher, if the current was turned on from the coil the ordinary secondary spectrum of hydrogen was obtained whether the tube was heated or cool. If the tube was kept hot at a little less than 2 mm pressure and the pressure gradually reduced, the hydrogen glow began to give way to the sodium discharge. A bright yellow ring of light, touching the inner wall of the tube, appeared over the molten sodium, and as the pressure decreased, this light became more and more intense, contracting toward the center of the tube as the cathode dark space widened. The whole of the discharge took place through the sodium vapor. The sodium light was most intense when it was scarcely possible to draw off any more gas through the pump. Then the discharge, viewed from the window at the end of the tube, appeared as an orange-yellow disk of almost insupportable brilliancy, usually completely detached from the wall of the tube.

This luminous disk evidently was the negative glow seen end-on, it being a solid cylinder along the axis of the tube, over the sodium. An image of the luminous disk was thrown on the slit of the spectroscope by means of a lens.

The nature of the discharge in the tube varies with the temperature, degree of exhaustion, and the potential applied to the electrodes. At a comparatively low temperature, when the yellow light of the sodium first appears, the negative glow had a curious appearance when viewed through the window in an oblique direction. There was an orange-yellow spot near the window, behind which the column of vapor was non-luminous; in the vicinity of the positive electrode on the other side of the dark region there was a green spot

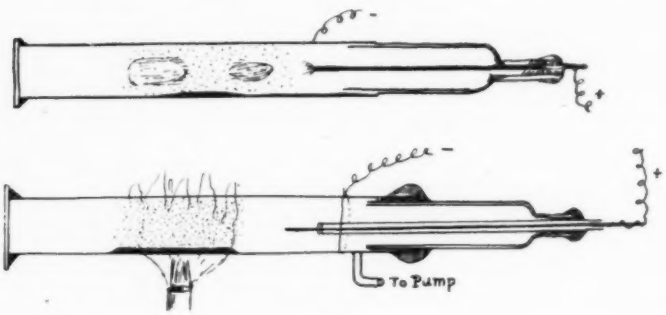

FIG. 1

of light (Fig. 1, upper figure). Viewed from the other end of the tube this spot of light was seen to be orange yellow, its apparent green color when seen through the window being evidently due to absorption. The reason why the central part of the column of vapor was non-luminous is not at once evident; but it may be that the current passed almost entirely through the vapor at the two ends of the column, where the vapor was diluted with hydrogen, the denser vapor carrying practically none of the current. This would presuppose that the sodium vapor conducts better if it is mixed with hydrogen. It is possible too that in the region where condensation is taking place the electrical properties of the vapor differ from those of the superheated vapor. Quite recently (*Physikalische Zeitschrift*, **24**, 1130, 1910) J. Koenigsberger has drawn

attention to the circumstance that with pure mercury in a very highly exhausted tube no discharge takes place even at a temperature of 150°, with a potential of 100,000 volts, either direct or alternating. It thus appears that metallic vapors do not conduct well unless mixed with small traces of some other gas.

CHARACTERISTICS OF THE SPECTRUM

1. *Red and yellow bands.*—Figs. 1 and 2 of Plate VI are from photographs made with the grating and show that the red and yellow portion of the cathode-ray fluorescent spectrum consists of a number of bands, sixteen of which are distinguishable on the negative. The negative of Fig. 1 was exposed for 30 minutes, that of Fig. 2 for three hours; with a longer exposure more bands could probably be obtained, since faint lines run out as far as λ 6864.

TABLE I

RED AND YELLOW BANDS

Band	Wave-Length	Difference	Band	Wave-Length	Difference
I	6565		X	6053	
		50 Å			53 Å
II	6515		XI	6000	
		60			54
III	6455		XII	5946	
		67			52
IV	6388		XIII	5894	
		60			51
V	6328		XIV	5843	
		52			51
VI	6276		XV	5792	
		58			48
VII	6218		XVI*	5744	
		54			44
VIII	6164		XVII*	5700	
		52			
IX	6112		Mean		54.7
		59			

* Bands not well defined.

The distance between the bands is approximately equal in a normal spectrum (see Table I), the mean difference between the centers of intensity of two adjacent bands being 54.7 $\mu\mu$. It is of course impossible to designate the exact center of any band. It is interesting to note that this spacing is of the order of magni-

tude of the spacing of the resonance fluorescent lines[1] excited in this region by the lithium lines λ 6104 and λ 6708.

These red and yellow bands are much less symmetrical than the green fluorescent bands excited by white light. The lines in these cathode-ray bands are irregularly distributed, giving no suggestion of a head or tail.

The best conditions for obtaining these red and yellow bands were: a high vacuum (about 0.1 mm); a fresh supply of sodium in the tube; high potential discharge; high temperature. The high potential was especially important. When the potential was rather low, nothing except the D lines and the lines of the subordinate series appeared; but when the e.m.f. of the coil was gradually increased, a critical value seemed suddenly to be reached at which the banded spectrum flashed out at almost full brilliancy. This e.m.f. was such as to give a spark of about seven inches in air. At high temperatures and low pressures it was sometimes found that the banded spectrum could be obtained with moderate potentials, but the brilliancy was not very great. On gradually increasing the potential, a critical point was reached at which the negative glow suddenly flashed with about tenfold intensity, becoming so bright as to be painful to the eye. If the potential was held at approximately this point, the bright discharge flashed intermittently, say once for every four or five vibrations of the interrupter, showing that the very slight accidental changes in the potential were sufficient to determine the occurrence or absence of the very brilliant discharge. It looks very much as if, for a given condition of the tube, a certain definite potential brings about a very complete ionization of the vapor, which enables it to carry a large current. If the potential drops below this value, by ever so small an amount, the ionization instantly falls to a very low value. No measurements were made of the amount of current flowing through the tube in the two conditions, a matter which should be investigated.

The red and yellow bands are made up of some 600 lines, the wave-lengths of which have been measured as a matter of record. In the present state of our knowledge regarding band spectra it

[1] R. W. Wood and F. E. Hackett, *Astrophysical Journal*, **30**, 339, 1909.

does not appear to be worth while to publish this table. Until such spectra can be decomposed into simple series, such as the resonance spectra discovered by one of the present writers, tables of the wave-lengths of the lines are of little or no value in adding to our knowledge. A comparison of these red and yellow bands with the absorption spectrum shows that there is no relation between the two spectra. Fig. 1 shows the spectra side by side, the barium arc lines being superposed upon the absorption spectrum. Fig. 1, on which the absorption spectrum was recorded with the cathode-ray spectrum, has been mounted in coincidence with Fig. 2, overlapping it a trifle in order to bring the absorption spectrum of Fig. 1 into position for comparison with Fig. 2, which shows the complete band spectrum. Short exposures were necessary to secure detail in the vicinity of the D lines, where the bands have a very great intensity. In Fig. 3 the absorption spectrum runs through the center of the cathode-ray spectrum.

A curious relation exists between the cathode-ray spectrum and the fluorescent spectrum excited by white light when the pressure of the nitrogen in the tube is about 1 cm. Fig. 6 shows that the *dark* bands of the cathode-ray spectrum coincide with the *bright* bands of the fluorescent spectrum. An entirely different fluorescence, excited by white light, in no way related to the cathode-ray spectrum, is obtained from sodium vapor when the nitrogen is at a pressure of only two or three millimeters, a matter which will require further investigation.

2. *Green bands.*—The portion of the cathode-ray spectrum lying between λ 5300 and λ 3053 is shown in Figs. 4 and 5 (Plate VI). A series of fourteen bands lies in the green, between λ 5200 and λ 4750. It is found that these coincide with the green fluorescent bands excited by white light. They appear under the same conditions as the red and yellow bands just described. Fig. 5 was made with a denser vapor than Fig. 4, and has been pasted over the iron comparison spectrum of the latter.

The coincidence of the green cathode-ray bands with the green fluorescent bands, together with the lack of agreement between the two spectra in the red, suggests that the appearance of the green bands in the cathode-ray spectrum may be a secondary effect. It

seems probable that the cathode rays excite the lines of the subordinate series (as shown in Fig. 5), which lines in turn excite by resonance the green fluorescence. The lines of the subordinate series between λ 5200 and λ 4500 are so numerous that the resonance spectrum due to them would appear almost identical with the white-light fluorescence spectrum. The absence of the resonance spectrum in the yellow and red is due to the fact that there are so few strong exciting lines in that region. This hypothesis as to the secondary origin of the green bands is supported by the fact that their intensity is much less than that of the red cathode-ray bands.

3. *Blue and violet bands.*—These bands are shown in Fig. 4; the blue band lying between λ 4541 and λ 4498, the violet band between λ 4390 and λ 4340. A weak continuation of the violet band extends as far as λ 4210. These bands are unresolved, and as we have been unable to identify them with any bands previously known, we feel disposed to refer them to sodium.

Since neither of these bands is resolved, they must be totally different from the bands found by Zickendraht[1] in this vicinity. His bands were in the spectrum of the negative pole of an arc burning in sodium vapor, at a pressure of 26 mm. He was enabled to resolve his bands with a prism giving about one-fourth the dispersion available in the present case.

Fig. 5 shows that these two bands appear under the same conditions as the red and green bands, for though the red bands do not appear in the figure, they are always very strong if there is any trace of the green bands.

4. *Ultra-violet bands.*—There are two other bands in this spectrum which lie at λ 3914 and λ 4273, each one of which consists of 10 or more well-defined lines. These are without doubt the nitrogen bands. In the plates taken with the quartz spectrograph these bands are unresolved. In some of these plates they are present with varying intensity; in other plates they are absent. There is a very strong line a little to one side of each band, which may be the head of the band. Fig. 4 shows that these bands come out strongest when the temperature is not high enough to give the other bands strongly; i.e., they are not present when the sodium vapor

[1] *Annalen der Physik*, **31**, 233, 1910.

is made dense by heating beyond a certain point. Even under these conditions they did not always appear. Their absence cannot be referred to absorption, since sodium vapor is extremely transparent in this region as has been shown by one of the present writers. The variable appearance is doubtless due to the fact that

TABLE II

λ	Intensity	λ	Intensity	λ	Intensity
5221.0	1	4308.7	1	3582	2
5153.7 } 2 S.	4	4278.2	6	3576	5†
5149.2 }	3	4273.0	1	3536.5	2
4983.5 } 1 S	10	71.5	1	3533.3	10
4979.3 }	10	70	1	3371	8†
4862	1	68	1	3327.6	2
4752.2 } 2 S	5	65	1 *	3318.2	2
4748.3 }	4	62	1	3304.8 } ‡	3
4710	1	59	2	3302.8 }	3
4668.4 } 1 S	9	56	1	3285.4	8
4664.7 }	8	52	1	3274.1	3
4544.8 } 2 S	3	4248	1	3257.9	6
4541.3 }	2	4236	2	3235.0	2
4497.3 } 1 S	6	3914	10	3225.8	1
4493.8 }	5	3908.7	2	3213.6	4
4490	1	3907	3	3189.4	4
4484.5	2	3905	3	3169.2	1
4481.5	2	3903	3 *	3163.9	4
4454.5	1	3901.3	3	3158.8	2
4448	1	3898.8	3	3149.0	3
4421.9 } 2 S	2	3896.6	2	3134.9	3
4418.6 }	1	3894.0	1	3128.9	6
4404.8	1	3884.8	2	3092.3	8
4392.8 } 1 S	4	3882.6	2	3078.9 }	1
4389.4 }	3	3805.1	3	3077.4 }	3
4344.0 } 2 S	3	3754.8	2	3073.9 }	2
4341.1 }	2	3710.8	5	3055.4 }	3
4324.7 } 1 S	3	3631.6	8	3052.9 }	3
4321.4 }	2				

* Nitrogen. † Winged. ‡ Principal series.

in some cases air was admitted to the tube during the progress of the experiments.

5. *Lines.*—In addition to the bands we have numerous bright lines in the sodium spectrum. Evidently a large number of these lines belong to the first and second subordinate series.

In the ultra-violet there is a series of lines some of which coincide apparently with the spark lines while others appear to be new.

These ultra-violet lines are shown by Fig. 7 (made with quartz spectrograph) to be strongest when the bands are weak, i.e., at moderate temperatures, a circumstance resulting probably from absorption. The wave-lengths were measured on the grating photograph (Fig. 4) and are as shown in Table II, the lines of the first and second subordinate series being designated by an *S*.

At a certain stage in the warming of the sodium the band spectrum was found to contain a number of strong lines in the orange which turned out to be the secondary lines of hydrogen. When a pressure of about 1 mm was reached, just when the yellow sodium light began to appear, these hydrogen lines came out strongly. Usually they disappeared as soon as a slightly higher vacuum was obtained; but once they were present for a considerable length of time and were recorded on a plate together with the sodium spectrum. This hydrogen spectrum could be obtained with the tube cold as in an ordinary hydrogen vacuum tube. In Fig. 3 these hydrogen lines appear (each marked with an *X*) superposed on the cathode-ray spectrum.

SUMMARY

A new spectrum is obtained from the negative glow of a vacuum tube containing sodium vapor. This spectrum shows the D lines, the first and second subordinate series, the green fluorescent bands excited by white light, a series of symmetrical bands in the red and yellow, two unresolved bands in the blue and violet, and a large number of new lines, some of which may however coincide with the spark lines. The red and yellow bands and the violet bands have been found in no other spectrum of sodium vapor.

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ON THE RATIO BETWEEN THE DIAMETER OF THE PHOTOGRAPHIC IMAGE OF A POINT AND THE EXPOSURE WHICH PRODUCED IT

By C. E. KENNETH MEES

It is well known that if a photograph be taken of an extremely small object, such as a star-image, or a spectral line, then the diameter of the image produced upon development depends upon the exposure given. This diameter is, even with short exposures, greater than the diameter of the star-image or spectral line, the increased diameter being produced by the phenomenon known as irradiation, due to the scattering of the light by the particles of silver-bromide composing the film of the plate.

This increase of diameter with exposure has been used by astronomers as the basis of a system of photographic photometry, the magnitude of faint stars being deduced from the relation between the diameters of their images and of the images of stars of known magnitude.

The formula which has been generally adopted as expressing the relation between intensity and diameter of image is that known as the Greenwich formula: $m=a-n\sqrt{D}$, which has been modified by various inserted terms to suit special conditions. From this $\sqrt{D}=a+b \log I$, where I is the intensity of the light.

This conclusion—that the square root of the diameter of a star-image is proportional to the logarithm of the exposure given—seemed so opposed to the general laws of the photographic plate, and to other work on irradiation, that the subject was investigated on a laboratory scale.

In the first place it must be premised that the diameter of a star-image is not a thing which can be measured with a high degree of accuracy. The best way of measuring it seems to be by the use of an eyepiece micrometer; deciding by the eye where the image shall be considered to start and where to leave off. The use of traveling microscopes certainly does not give results of equal accuracy.

The object for these experiments consisted of a spectroscopic slit which could be limited as to height by a *V*-plate sliding over it, so that a small, nearly square, opening could be obtained. This slit was photographed from a distance of 3 meters by a lens of 15 cm focus, the reduction on the plate being exactly 20:1. The slit was lighted by a Nernst burner and condenser and exposed for varying lengths of time, the diameter of the images on the developed plate being read as divisions of an eyepiece micrometer in a microscope. One division of this micrometer was equal to 0.0075 mm. The accuracy of reading the diameters is not greater than one division, and for great diameters rather less.

The following results were obtained: slit 7 mm long, 1/10 mm wide. The calculated diameters are inserted, calculated on the assumption that $d = a + b \log E$.

Plate Used	Exposure	Diameter	Differences of Diameters	Diameter (calculated for $\Delta D = 2\frac{1}{2}$)
Fine-grained bromide Lantern plate	5	4		4
	10	7	3	6.5
	20	9.5	$2\frac{1}{2}$	9
	40	12	$2\frac{1}{2}$	11.5
	80	14.5	$2\frac{1}{2}$	14
			(Calculated for $\Delta D = 4$)	
Process plate	5	15		15
	10	18	3	19
	20	21	3	23
	40	24	3	27
	80	28	4	31
	160	33	5	$35\frac{1}{2}$
	320	39	6	39
	640	43	4	43
	1,280	47	4	47
	2,560	52	5	51

This experiment is noteworthy because the range of exposures varies from 1 to 512. The effect of doubling the exposure is not quite constant throughout the range, being apparently 3 at first and then increasing to about $4\frac{1}{2}$ for the remainder of the scale.

Slit 1 mm wide, 7 mm long:

Plate Used	Exposure	Diameter	ΔD	Diameter (calculated for $\Delta D = 2\frac{1}{2}$)
Lantern	1	16		16
	2	18	2	$18\frac{1}{2}$
	4	20	2	21
	8	23	3	$23\frac{1}{2}$
	16	28	5	26
	32	31	3	$28\frac{1}{2}$
	64	32	1	31
	128	34	2	$33\frac{1}{2}$

Slit $\frac{1}{2}$ mm wide, 7 mm long:

Plate Used	Exposure	Diameter	ΔD	Diameter (calculated for $\Delta D = 3$)
Lantern	1	9		9
	2	12	3	12
	4	14	2	15
	8	18	4	18
	16	20	2	21
	32	23	3	24

Square aperture, 1 mm diameter:

Plate Used	Exposure	Diameter	ΔD	Diameter (calculated for $\Delta D = 2\frac{1}{2}$)
Process	$\frac{1}{2}$	18		18
	1	20	2	$20\frac{1}{2}$
	2	22	2	23
	4	24	2	$25\frac{1}{2}$
	8	26	2	28
	16	29	3	$30\frac{1}{2}$
	32	33	4	33
	64	36	3	$35\frac{1}{2}$

Square aperture, 3 mm diameter:

Plate Used	Exposure	Diameter	ΔD	Diameter (calculated for $\Delta D = 3$)
Process	$\frac{1}{2}$	35		35
	1	38	3	38
	2	40	2	41
	4	42	2	44
	8	45	3	47
	16	48	3	50
	32	53	5	53

These results seem to show quite conclusively that the diameter of the small image of a fine slit or point is proportional to the logarithm of the exposure given.

The equation used for astronomical work is therefore not based on a law of the photographic plate, but upon some modification of the true law dependent on the conditions of the formation of images in telescopes.

NOTE.—It has already been shown by Dr. W. Scheffer that, in general, the spreading of light from a point in a turbid medium is an exponential function of the intensity, and in a private communication he states that he has independently found that the diameter of a point-image is proportional to the logarithm of the exposure. His results are, consequently, in accord with those of the author.

RESEARCH LABORATORY OF
WRATTEN & WAINWRIGHT, LTD.
CROYDON, ENGLAND
December 15, 1910

Minor Contributions and Notes

ADDITIONAL SECONDARY STANDARDS, INTERNATIONAL SYSTEM, IN THE ARC SPECTRUM OF IRON

At the meeting of the International Union for Co-operation in Solar Research held at Mount Wilson, August 31, 1910, the Committee on Wave-Lengths was authorized to publish from time to time wave-lengths which should serve as secondary standards of the International System when these lines had been measured by three independent observers, provided the measurements were satisfactory. The committee reports now that it wishes to add to the list published in the October number of the *Astrophysical Journal* the following lines:

Adopted Wave-Lengths	Fabry and Buisson	Eversheim	Pfund
4352.741	.741	.741	.741
4859.758	.756	.758	.759
4966.104	.104	.105	.103
5302.315	.316	.316	.314
5324.196	.196	.196	.195

These lines all are taken from the arc spectrum of iron.

H. Kayser
Ch. Fabry
J. S. Ames

December 27, 1910

"ON THE TEMPERATURES OF THE STARS"

I am glad to see from Mr. Abbot's note in the November number of this *Journal* (**32**, 319, 1910) that our views have come into agreement up to the point of a title, which is not particularly important. I nevertheless wish to recall to attention that the memoir, *Temperaturbestimmung von 109 helleren Sternen aus*

spectralphotometrischen Beobachtungen, as was expressly stated therein, deals only with the determination of the *effective* temperatures; or attempts to treat of the purely formal question whether the constant T of the Kirchhoff function can be so determined that the observations in the visual part of the stellar spectrum can be properly represented.

As to the solar spectrum, the excellent bolometric measures by Abbot and Fowle on Mount Wilson were represented, in the region between $0.55\,\mu$ and $1.92\,\mu$ (excepting the portion of the energy-curve between $0.3\,\mu$ and $0.5\,\mu$, which is deformed by the numerous absorption lines), with systematic deviations of only about twice the precision attainable in spectral-photometric measurements.

The value for the sun $T = 5990°$, or $T = 5830°$, according as the constant of radiation c was taken as 14,600 or 14,200, is in good agreement with the results of the pyrometric measurement by Abbot and Fowle, $T = 5962°$; with the pyrheliometric measures of Scheiner, $T = 6200°$; and with the spectral-photometric measures of Scheiner and Wilsing, $T = 5600°$.

J. WILSING

POTSDAM
December 1910

CORRECTION

In our paper on "The Velocity of the Sun's Motion through Space as Derived from the Radial Velocity of *Orion* Stars," in the July number of this *Journal*, this statement is made on p. 89:

> Our result, however, is not explained by the formula of the last-named astronomers, Hough and Halm, for that gives an identical mixture for any two points of the sphere diametrically opposite.

This statement is incorrect, and is quite inexcusable. As a matter of fact the formula of Hough and Halm gives generally a different mixture for two opposite points of the sky. We thank Professor Schwarzschild for calling our attention to the error.

J. C. KAPTEYN AND EDWIN B. FROST

October 1910

Reviews

Handbuch der Spectroscopie, Band V. Von H. Kayser. Leipzig: S. Hirzel, 1910. Large 8vo, pp. 853, with two plates and three figures. M. 48; bound, M. 52.

If the individual volumes of this work had been given separate titles, this one, which marks the fifth step toward completion of the one indispensable work on spectroscopy, would doubtless be called "The Spectra of the Elements."

With characteristic directness the author puts into his very first sentence the plan and purpose of the entire book: "In these volumes has been collected all our spectroscopic information concerning the various elements in order to give the reader the most complete picture possible of what has already been accomplished and of what remains to be done." It is, in fact, the first orderly and comprehensive summary and discussion of results in a science which has already celebrated its jubilee.

The mode of treatment is briefly as follows. The elements are taken up in the alphabetical order of their symbols, the first fifty being covered by the present volume. Of these, six are discussed by Professor Kayser's friend and former colleague, Professor Konen of Münster.

Under each element is given, first, the bibliography of the spectra of that element. Following each citation comes generally a single word—sometimes two—such as "spark," "self-reversal," "banded spectra," "measurements"—giving the keynote of the paper referred to. These references sometimes exceed two hundred for a single element. They are not absolutely, but essentially, complete.

Next comes the discussion of results, occupying for a single element anywhere from one to forty pages. Here, in general, one finds an orderly historical presentation of what is known concerning the line spectra, in arc, spark, and flame, the banded spectra, and the spectra of compounds of this particular element. By means of bracketed numerals references are given, in the text, for practically every statement of fact. The reader is thus at once in possession of the author's authority, a feature which illustrates the impartial, judicial, and scientific spirit which pervades the entire work. On the other hand frank expressions

of opinion and directness of speech are not wanting. Witness the following from a single page: "In my opinion this is entirely incorrect." "The conclusion, that . . . . has two spectra, is thoroughly unjustified." ". . . . claims. . . . . It is certainly not true."

The whole discussion deals with the successive accomplishments of individual men. Our knowledge of the spectra of each element is presented as a series of human achievements; and in this regard has much of the interest which attaches to political and literary history.

Now and then, though rarely, some hitherto unpublished facts are slipped in, as for instance Eversheim's accurate measures on helium.

To follow Professor Kayser's summary concerning any one of the more important elements, such as potassium, iron, or calcium, is, in a certain sense, to follow the whole history of spectroscopy. For under each such element one is sure to meet remarks on the various sources of radiation, the different instruments of dispersion, the methods of recording and measuring spectra, and the logic of inferences to be drawn from direct observations. All these combine to make the volume much more than a catalogue of the ships. One finds it, on the contrary, a series of sketches representing spectrum analysis from as many points of view as there are important elements. In following these comments, one is forcibly reminded of how much room remains for honest difference of opinion—or, better, for suspension of opinion—upon many of the fundamental questions of spectroscopy: for instance, the origin of the so-called "aluminium-oxide bands," or even the origin of banded spectra, in general.

The third and last step in the treatment of each element is the tabulation of wave-lengths. The superficial reader will here be impressed by the fact that a considerable number of obsolete and imperfect measures are included in these tables. But all these have a qualitative value as showing just what a certain observer saw under his working conditions. Besides it must be admitted that, so far as mere precision is concerned, *all* wave-length determinations previous to the beginning of the present century, with the exception of Michelson's interferometer measures, are obsolete.

Apparently the only element in this entire volume for which the wave-lengths are given consistently and with any completeness in terms of the International Ångström is that of mercury by Stiles.

The appearance of the next volume of this compendium will, in a certain sense, complete the literature of a definite era in the history of

metrical work on spectra. In this era the publication of Kirchhoff and Bunsen's researches in 1860 may be said to form the first chapter.

The appearance of Ångström's map of the solar spectrum, with its introduction of the tenth-meter, in 1868 marks the second step. The third chapter begins with Rowland's Table of Standard Wave-Lengths (1887 and 1889) and the appearance of his solar map.

We stand on the shoulders of our predecessors. And, as we now see it, the fourth chapter began with the introduction of interference methods, and in particular with Michelson's evaluation of the meter in 1892. A delay of ten or fifteen years in the acceptance of Michelson's value by spectroscopists seems to have been due to the fact that the reason for the discrepancy between grating and interferometer methods had not been explained. With the perfection of the interferometer and the adoption of an arbitrary, international standard of a new order of accuracy—say, 0.001 Å or one part in six million—the science of spectral measurement stands at the threshold of the future and at the beginning of chap. v.

Chemists will be interested in three brief preliminary chapters, pp. 11–31, in which the spectroscope is shown to have little value in either qualitative or quantitative analysis in the sense in which these terms are generally used.

A publisher who will undertake the issue of six large volumes devoted to a science so utterly "useless" (from the point of view of "the man on the street") as spectroscopy is at once a patron of, and an honor to, pure science. The joint service of publisher and author is not likely to be over-estimated.

HENRY CREW

NOTICE

The scope of the ASTROPHYSICAL JOURNAL includes all investigations of radiant energy, whether conducted in the observatory or in the laboratory. The subjects to which special attention is given are photographic and visual observations of the heavenly bodies (other than those pertaining to "astronomy of position"); spectroscopic, photometric, bolometric, and radiometric work of all kinds; descriptions of instruments and apparatus used in such investigations; and theoretical papers bearing on any of these subjects.

Articles written in any language may be accepted for publication, but unless a wish to the contrary is expressed by the author, they will be translated into English. Tables of wave-lengths will be printed with the short wave-lengths at the top, and maps of spectra with the red end on the right unless the author requests that the reverse procedure be followed.

Accuracy in the proof is gained by having manuscripts typewritten, provided the author carefully examines the sheets and eliminates any errors introduced by the stenographer. It is suggested that the author should retain a carbon or tissue copy of the manuscript, as it is generally necessary to keep the original manuscript at the editorial office until the article is printed.

All drawings should be carefully made with India ink on stiff paper, usually each on a separate sheet, on about double the scale of the engraving desired. Lettering of diagrams will be done in type around the margins of the cut where feasible. Otherwise printed letters should be put in lightly with pencil, to be later impressed with type at the editorial office, or should be pasted on the drawing where required.

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Authors are particularly requested to employ uniformly the metric units of length and mass; the English equivalents may be added if desired.

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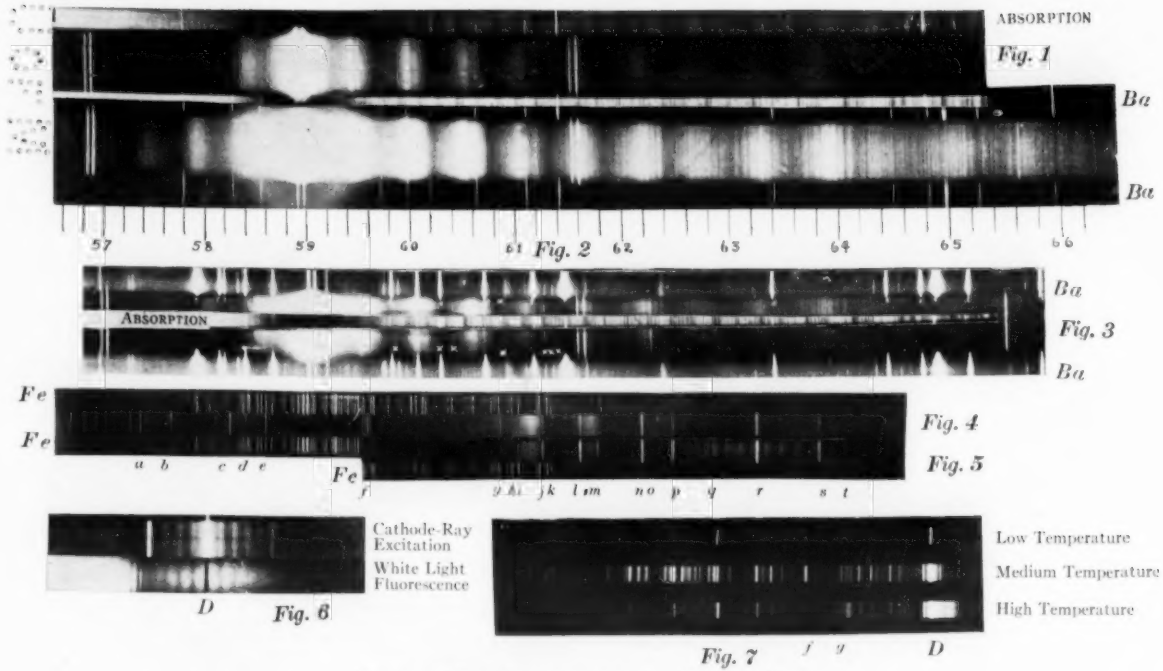
PLATE VI



CATHODE-RAY FLUORESCENCE OF SODIUM VAPOR

Wave-lengths corresponding to letters in Figs. 4, 5, and 7:

a, 3285	*e*, 3631	*i*, 4344	*m*, 4544	*q*, 4802
b, 3371	*f*, 3914	*j*, 4302	*n*, 4668	*r*, 4983
c, 3533	*g*, 4278	*k*, 4421	*o*, 4710	*s*, 5154
d, 3576	*h*, 4324	*l*, 4407	*p*, 4752	*t*, 5221